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VOL. XLIV. JULY—DECEMBER, 1861.

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

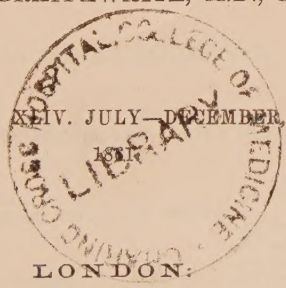
EDITED BY

W. BRAITHWAITE, M.D.,

LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D., LOND.



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CONTENTS OF VOL. XLIV.

SYNOPSIS.

A COMMENTARY ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ARTICLE.		AUTHOR.	PAGE.
1	On Fever in the Zambesi	*Dr. Livingstone*	1
2	On the Difference between *Curing* and *Treating* a Fever	*Dr. P. M. Latham*	2
3	On Typhus Fever	*Dr. Bell*	6
4	Fever in the Crimea	*Dr. Lyons*	9
5	Pathognomonic Sign of Scarlatina	*M. Bouchut*	9
6	On the Action of Sesquicarbonate of Ammonia in Scarlet Fever	*Dr. John M'Nab*	10
7	On the Use of Benzoate of Ammonia in Renal Dropsy succeeding Scarlatina	*Dr. J. Taylor*	11
8	On Arsenious Acid as a substitute for Quinine in Fever	*J. Turner, Esq.*	12
9	On Actea Racemosa in Acute Rheumatism	*Dr. F. R. Mac Donald*	12
10	Case of Acute Rheumatism, with unusual Complication	*Prof. Reid*	13
11	Medicinal Treatment of Gout	*M. Trousseau*	16

DISEASES OF THE NERVOUS SYSTEM.

ARTICLE.	AUTHOR.	PAGE.
12 On Acute Softening of the Medulla Spinalis	*Dr. Robert Mayne*	16
13 On Reflex Paraplegia	*Ed. Brit. Med. Journal*	18
14 Some Remarks on Paralysis	*Dr. C. E. Brown-Sequard*	25
15 Treatment of Delirium Tremens by large Doses of Tincture of Digitalis	*Dr. Francis E. Carey*	29
16 Digitalis in Delirium Tremens	*Dr. D. M. Williams*	31
17 Delirium Tremens treated by Large Doses of Tincture of Digitalis	*Dr. Thos. B. Peacock*	32

ORGANS OF CIRCULATION.

18 On Blood-letting in Apoplexy	*Dr. T. K. Chambers*	35

ORGANS OF RESPIRATION.

19 On the Uncertainty of the Sputum as a Diagnostic Feature in Pneumonia	*Dr. Thos. Williams*	40
20 Chlorate of Potash and Sugar in Phthisis	*Dr. James Turle*	45
21 New Remedy for Whooping-Cough	*M. Foster, Esq.*	47
22 On Arsenic Smoking in Asthma	*Dr. Fred. G. Julius*	47
23 Drill for Auscultation	*Dr. T. K. Chambers*	48

ORGANS OF DIGESTION.

24 Curious Cases of Furred Tongue on one Side, from various Causes	*John Hilton, Esq.*	52
25 On Diphtheria	*Dr. William Budd*	56
26 On the Treatment of Acute Dysentery by Ipecacuanha	*Dr. R. W. Cunningham*	57
27 On the Treatment of Diarrhœa by Nitric Acid and Opium	*Dr. Patrick J. Hynes*	60

URINARY ORGANS.

28 On the Diseases of the Kidney, accompanied by Albuminuria	*Dr. C. J. B. Williams*	62
29 On Uræmic Intoxication	*Dr. W. A. Hammond*	64
30 The Urine in Albuminuria	*Dr. Lionel S. Beale*	66
31 On a New Clinical Method of estimating the Quantity of Sugar in Diabetic Urine	*Dr. William Roberts*	67
32 New Test for Saccharine Urine		72
33 On Anasarca	*Dr. Charles Witt*	73

SURGERY.

AFFECTIONS OF BONES AND JOINTS, &c.

ARTICLE.	AUTHOR.	PAGE.
34 On Amputations	*Thos. P. Teale, Esq.*	75
35 On the Influence of Rest in the Treatment of Diseases of the Joints	*John Hilton, Esq.*	77
	W. Joseph Square, Esq.	83
36 On Loose Cartilages in the Knee-joint	*Prof. Fergusson*	88
37 Excision of the Knee-joint;—Recovery with a False Joint, and a Useful Limb		
38 Treatment of Housemaid's Knee by the Thread Suture	*Fred. C. Skey, Esq.*	88
39 Excision of a Portion of the Os Calcis for Caries	*Dr. J. K. Barton*	89
40 Pirogoff's Operation	*Prof. Fergusson*	92
41 On Dislocations of the Shoulder-joint	*Dr. N. R. Smith*	93
42 On the Treatment of Fracture of the Lower Extremity of the Radius	*Dr. Alex. Gordon*	97
43 Complete Restoration of the Tibia by the Periosteum, after its Removal	*M. Maisonneuve*	103
	William Adams, Esq.	105
44 On Anterior and Posterior Curvatures of the Spine		

ORGANS OF CIRCULATION.

45 Traumatic Aneurism of the Femoral Artery	*Dr. Thos. J. Rawson*	112
46 On the Treatment of Popliteal Aneurism by Compression		114
47 Large Gluteal Aneurism; Ligature of the Gluteal Artery	*Prof. Syme*	114
48 The Treatment of Varicose Veins and Ulcers by Means of Blisters	*—— Ure, Esq.*	116
49 On the Surgical Treatment of Vascular Tumours	*P. C. Price, Esq.*	117
50 On the Radical Cure of Varicose Veins	*John Wood, Esq.*	123
51 A Bullet Lodged in the Heart for Twenty Years	*Dr. Balck*	124

ALIMENTARY CANAL.

52 A Rare Instance of Conservative Surgery	*Dr. J. Nichols*	125
53 Ulcerated Throat	*Ellis Westcott, Esq.*	126
54 On the Mode of Operating for the Radical Cure of Hernia	*John Wood, Esq.*	126
	Prof. Chisholm	128
55 On a New Operation for the Radical Cure of Hernia		
56 Complicated Case of Oblique Congenital Inguinal Hernia	*Dr. Wm. J. Wilson*	130
	M.M. Velpeau and Verneuil	135
57 Perforation of the Intestine	*James Lane, Esq.*	138
58 On some Diseases of the Rectum	*Henry Smith, Esq.*	141
59 On the Treatment of Certain Forms of Prolapsus Ani where the Ligature is not Admissible	*William Coulson, Esq.*	149
60 On Flat Ulcers of the Rectum	*Fred. C. Skey, Esq.*	149
61 On Abdominal or Pelvic Abscess		

ORGANS OF URINE AND GENERATION.

ARTICLE.		AUTHOR.	PAGE.
62	On Incisions for Stricture Compared with Dilatation	*John Gay, Esq.*	152
63	On the Immediate Treatment of Stricture of the Urethra	*Barnard Holt, Esq.*	154
64	Treatment of Stricture of the Urethra by Sudden and Forcible Dilatation	*Christopher Heath, Esq.*	163
65	On Incision of Stricture of the Urethra	*Dr. James Arnott*	166
66	On Phymosis	*P. C. Price, Esq.*	173
67	Lithotomy,—Lateral Operation	*Prof. Fergusson*	174
68	Radical Cure of Varicocele	*Henry Lee, Esq.*	175
69	On the Radical Cure of Varicocele	*J. Redfern Davies, Esq.*	176
70	New Remedy for Irritable Conditions of the Bladder	*Henry Thompson, Esq.*	177

DISEASES OF THE SKIN.

71	On Tubercular Disease of the External Lymphatic Glands	*P. C. Price, Esq.*	178
72	On the Treatment, without Excision, of Wens and of some other Cysts	*Prof. Courty*	184
73	On the Prevention of Pitting in Small-pox, by the Application of Linimentum Aquæ Calcis and Cotton Wool	*Dr. Joseph Bell*	188
74	Observations on the Treatment of Deformities Resulting from Severe Burns	*Dr. J. K. Barton*	190
75	Treatment of Scabies by Benzine	*J. J. Godfrey, Esq.*	199
76	On the Treatment of Phthiriasis	*Dr. T. McCall Anderson*	200
77	On the Treatment of Eczema	*Prof. Hebra*	201

SYPHILITIC AFFECTIONS.

78	On Syphilis		
79	Treatment of Gonorrhœa by Injections of Liquor Hydrargyri Pernitratis	*Henry Lee, Esq.*	203
80	Subnitrate of Bismuth as an Injection in Acute Gonorrhœa	*G. Borlase Childs, Esq.*	209
81	Gonorrhœa	*Dr. Mouslon*	210
82	On the Cure of Gonorrhœa by Blisters	*Dr. T. K. Chambers*	211
83	Chlorate of Potash in Gonorrhœa	*H. Chalmers Miles, Esq.*	211
84	How is Syphilitic to be Distinguished from Non-Syphilitic Psoriasis	*Dr. Irvin*	216
		Dr. T. M'Call Anderson	217

DISEASES OF THE EYE AND EAR.

85	On the Relief of Symblepharon by Transplantation of Conjunctiva	*T. P. Teale, jun., Esq.*	219
86	Presbyopia	*J. S. Wells, Esq.*	220
87	Black Eye		221
88	On Catheterism of the Eustachian Tube, and on the Pharyngoscope	*Dr. Voltolini*	221

LIBRARY

MIDWIFERY,

AND THE DISEASES OF WOMEN, &c.

ARTICLE.		AUTHOR.	PAGE.
89	On the Induction of Premature Labour	*Dr. J. Y. Simpson*	227
90	Indications and Operations for the Induction of Premature Labour, and for the Acceleration of Labour	*Dr Robert Barnes*	231
91	Induction of Premature Labour	*Dr Arthur Farre*	236
92	On the Treatment of Cases of Abortion, in which the Membranes and Placenta are Retained	*Dr. W. O. Priestley*	237
93	Accidental Separation of the Placenta at the Seventh Month of Pregnancy	*G. E. Stanger, Esq.*	239
94	The Treatment of Placenta Prævia	*Dr. Robert Barnes*	241
95	Observations on Placenta Prævia	*Dr. Charles Clay*	248
96	On a Peculiar (Syphilitic?) Degeneration of the Decidua	*Prof. Virchow*	251
97	What Influence has Contraction in Stopping Uterine Hemorrhage	*Dr. James Adams*	252
98	On Three Cases of Puerperal Convulsions ..	*Dr. Robert Harper*	253
99	Use of Chloroform in Puerperal Convulsions ..	*Dr. Fred Page*	255
100	On a Case of Epileptic Puerperal Convulsions ..	*Dr. F. W. Wilson*	256
101	On Supporting the Perineum in Labour	*Dr. Grailly Hewitt*	257
102	On Laceration of the Perineum	*Dr, James Gray*	259
103	On Supporting the Perineum in Labour	*Dr. E. W. Murphy*	260
104	On Rupture of the Perineum During Labour ..	*Dr. Thos. Skinner*	262
105	Is the Ergot of Rye, when Administered to the Mother during Labour, dangerous or not to the Life of the Child?	*Dr. Uvedale West*	264
106	On the Use of the Forceps in Tedious Labour ..	*Dr. G. Hamilton*	266
107	On a Modified Form of Obstetric Forceps, to be used in Cases where the Ordinary Ones are Inapplicable, owing to Unusual Elongation of the Fœtal Head..	*Dr. Grailly Hewitt*	269
108	Some Practical Observations on the Timely Use of the Obstetric Forceps .. : ..	*Dr. Edward Sinclair*	272
109	On the Obliquity of the Fœtal Head in the Mechanism of Parturition	*Dr. J. M. Duncan*	280
110	An Effort to Shorten the Duration and Diminish the Pain of the First Stage of Labour	*Dr. B. Fordyce Barker*	291
111	Spontaneous Evolution..	.. ..	295
112	Impeded Labour from Dorsal Displacement of the Arm	*J. J. Murray, Esq.*	296
113	On Uterine Hemorrhage	*Dr. G. Hamilton*	299
114	Extra-Uterine Gestation	*Dr. George Cheesman*	301
115	Improvement in the Speculum	.. ..	305
116	On Vesico-Vaginal and Recto-Vaginal Fistulæ ..	*T. Spencer Wells, Esq.*	306
117	On a New and Successful Mode of Treating Vesico-Vaginal Fistula	*M. H. Collis, Esq.*	309
118	On Secondary Abscess with Disorganization of the Knee-Joint, following Scarlatina and Parturition	*W. Coulson, Esq.*	315
119	Operation for and Perfect Cure of Procidentia Uteri from Perineal Laceration of many Years' Standing	*Dr. D. Lloyd Roberts*	320
120	On Abdominal Collapse	*Dr. Robert Barnes*	322
121	On Diphtheritic Ulcer of the Uterus	*Robert Ellis, Esq.*	329
122	On Inflamed Ulcer of the Uterus	*Do,*	332
123	On Removal of the Cervix Uteri . ..	*Dr. R. Greenhalgh*	336
124	On the Effects of Undue Application of Caustic Potash to the Uterus	*Dr. Whitehead*	337

ARTICLE.	AUTHOR.	PAGE.
125 On Ovariotomy and its Results	*Dr. Charles Clay*	338
126 On Ovariotomy	*T. Spencer Wells, Esq.*	348
127 Hysteria Masking Secondary Syphilis	*W. Coulson, Esq.*	350
128 On Irritation of the Urethra in Females	*John Hunter, Esq.*	350
129 Explanation of Hysterical Pains in the Hip or Knee-Joint	*John Hilton, Esq.*	351
130 On Leucorrhœa and Vaginitis	*Dr. Charles Waller*	353
131 Electricity in Amenorrhœa	*Dr. Althaus*	357
132 On the Local Treatment of Amenorrhœa	*Dr. J. Y. Simpson*	358
133 Case of Dysmenorrhœal Membrane	*Dr. Beatty*	362
134 On Dysmenorrhœa and Sterility	*Dr. John Coghlan*	364
135 On Inflammation of the Breast, with an Analysis of Seventy-two Cases	*T. W. Nunn, Esq.*	366
136 On Saccharine Fermentation in the Milk within the Female Breast	*Dr. George D. Gibb*	368
137 Weight of New-Born Infants	*Dr. E. von Siebold*	369
138 Researches on Asphyxia with Observations on the Effects of the Hot Bath	*Dr. A. T. H. Waters*	371
139 The Atrophy of Children from Dirt Eating	*Dr. William Moore*	374
140 On Cerebral Disease Unattended by Cerebral Symptoms	*Dr. Charles West*	377

MISCELLANEOUS SUBJECTS.

141 On the Use of Alcoholic Stimulants in Hospital Practice	*Dr. W. T. Gairdner*	383
142 On the Action and Uses of Alcohol	*Dr. Daniel Hooper*	386
143 On Veratrum Viride	*Dr. Otterson*	392
144 On the Galvano-Caustic Apparatus	*Dr. T. J. Walker*	395
145 On the Influence of Ozone on Organic Substances	*Dr. T. K. Hornidge*	399
146 Ferri Carbonas Effervescens; A New and Elegant Form of Chalybeate	*Dr. Thomas Skinner*	399
147 On the Chronometry of Life and Disease	*A. de Nöe Walker, Esq.*	401
148 On Internal Obstruction by Bands	*John Gay, Esq.*	405
149 Crinoline Accidents	*Dr. Odling*	408
150 Experiments Relating to the Diuretic Action of Colchicum	*Dr. W. A. Hammond*	409
151 The Hot-Bath: its Physiology, Use, and Abuse: especially in Connexion with the Treatment of Drowning	*Charles Hunter, Esq.*	413
152 Rules for Guidance in Chloroform Accidents	*Dr. Kidd*	416
153 On the Treatment of Gout	*M. Trousseau*	418
154 Formula for the Administration of Chloroform, Ether, Turpentine, Camphor, or Essential Oils	*M. Vee*	421
155 Essence of Beef	*Dr. Druitt*	421

INDEX.

A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—In the treatment of acute rheumatism by means of large doses of alkalies, care must be taken that the bowels be well opened. If this is not the case, large quantities of alkali are retained in the system, and sometimes produce unpleasant effects. (Prof. Reid, p. 13.)

Give twenty-five to forty drops of the tincture of the *Actea racemosa* three times a-day. It may be obtained of Messrs. Duncan and Flockhart, Edinburgh. It is almost uniformly successful in both acute and subacute rheumatism. (Dr. F. R. Mac Donald, p. 12.)

GOUT.—The waters of Vichy, Vals, and Carlsbad, do more harm than good in gout. Waters so *strongly* impregnated with alkali are inconsiderately prescribed in the present day. But the waters which are *feebly* alkaline do much good; these are to be found at Pongues, Contrexéville, Plombières, Spa, and Wiesbaden. When your patient has gravel as well as gout, send him to Contrexéville or Pongues. Alkaline saturation provokes the appearance of atonic and visceral gout. It is better to accustom the surface to cold, by means of bathing or the cold douche, than to keep it too warm by clothing. Also avoid too much animal food, and take a fair amount of exercise. (M. Trousseau, p. 418.)

In the course of the twenty-four hours give two or three of the following pills for three or four successive days. Quinæ sulphatis, gr. xxij; ext. seminis colchici, gr. viij; ext. digitalis, gr. iv. M. divide in pilulas decem. (M. Trousseau, p. 16.)

INTERMITTENT FEVER.—You may often succeed as well, and even better, by giving arsenious acid in these cases, than by the use of quinine. Give it as follows: Arsenite of potash, and compound tincture of cardamoms, of each half a drachm; gum mucilage, three drachms; camphor mixture or water, half-an-ounce; mix. To be given every second hour for four or five times, the last dose to anticipate the expected paroxysm at least two hours. Even to a baby

nine months old ℥xx. of the liq. potassæ arsenitis may be given in ten hours, repeating the dose next day if necessary. (Mr. J. Turner, p. 12.)

Malarious Fever.—Mix a purgative with the quinine in the treatment of these cases, as follows: Resin of jalap, calomel, of each eight grains; quinine and rhubarb of each four grains; mix well together and make into pills. Dose from ten to twenty grains. This medicine brings away a great quantity of black bile almost like blood. Sometimes you have to give a dessert-spoonful of epsom salts, when the above does not operate sufficiently. Quinine must next be given till the *ears ring*. The above doses are for strong adults. (Dr. Livingstone, p. 1.)

Scarlet Fever.—Use the sesquicarbonate of ammonia as a gargle, as follows: Sesquicarbonate of ammonia, two drachms; water six ounces. Gargle the throat with this every four hours. Also give it internally in five or ten-grain doses, according to age, three times a day. (Dr. J. Mc.Nab, p. 10.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Delirium Tremens.—In a case related by Dr. Carey, of Guernsey, opium aggravated the symptoms; whilst half an ounce of tincture of digitalis, with an equal quantity of gin, caused the patient to fall into a deep sleep, which continued without intermission for twelve hours. He awoke quite rational, and in two days was quite well. The digitalis did not produce its alleged results, diarrhœa and vomiting, nor were the kidneys stimulated to increased action. The influence of the drug on the pulse was not such as to offer any barrier to its repetition. (Dr. F. E. Carey, p. 29.)

The large doses of tinct. digitalis, half-ounce doses, at long intervals of twenty-two or twenty-four hours, may be given safely. The pulse falls, but acquires power and steadiness, and the tremor subsides. It is best to confine its use to young and robust subjects, whose strength has not been broken-down by prolonged habits of intemperance, and to cases of excessive spirit drinking. (Dr. T. B. Peacock, p. 32.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Apoplexy.—The first effect of an apoplectic seizure is a violent shock to the nervous system. The skin becomes damp and cold and pale, the pulse and heart beat quickly and irregularly. If the patient be bled at this time fatal syncope might ensue, but after a time the heart recovers its power; if this take place we must bleed to prevent a return of the cerebral hemorrhage. In some forms of apoplexy these symptoms do not occur—as in that form

depending on destructive softening of the cerebral substance. In such a case bleeding would aggravate the symptoms materially. But in all cases we may safely be guided by the pulse: if it be sharp and hard at the wrist it will be sharp and hard in the brain. (Dr. T. K. Chambers, p. 35.)

Varicose Veins.—By applying repeated blisters over varicose veins of the leg, you cause deposition of fibrine in the surrounding areolar structures, and partial obliteration of the enlarged vein. The vein of course ceases to be varicose, and any ulcers present heal. (Mr. Ure, p. 116.)

Use Mr. Wood's pair of needles, which are of the thickness of an ordinary hare-lip pin, and bent at right angles, about an inch from the head, forming, at the bend, a loop for the reception of the point of the fellow needle. The head, or blunt extremity, is also formed into a loop to serve for the purpose of retention by a strip of lint or plaster. The point of one needle is straight and spear-shaped, as in the hare-lip pin; that of its fellow is curved as in the ordinary surgical needle for sewing up a wound. The method of application is as follows:—The skin over the vein being pinched up and raised from the vein by the finger and thumb, the straight needle is passed close under the skin, between it and the vein. The finger and thumb are then adjusted so as to raise the vein from the fascia a little on one side of the needle. The curved needle is then passed through the same punctures in the skin as the former one, but in an opposite direction and under the vein, which is thus included between the needles. The needles are then locked on to each other by passing the points of each through the loop at the bend of the other. The punctures are then protected from pressure by the interposition of a piece of lint or wash-leather, and the needles pushed in so that the loops are close to, but do not press upon the punctures. The points of the needles are then cut off with pliers. The bent head of one needle is then turned upon its fellow until it lies on the limb in the same direction. This will be found to twist the shafts across each other in such a manner as to compress laterally the included vein, and at the same time slightly to twist it. The needles are then held firm in their places by a strip of lint twisted figure-of-eight-wise, and a strip or two of sticking-plaster, care being taken to prevent all chafing. They are left in a week or ten days, until so much effusion has taken place as permanently to obstruct the current in the vein, and by progressive absorption from *acupressure on both sides* to cause complete obliteration. In the many cases treated by Mr. Wood in this manner, there has been no suppuration whatever in the track of the needles, when care has been taken to prevent chafing or irritation. A drop or two of serum, forming a sugar-like crust, is the only discharge. (Mr. J. Wood, p. 123.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

Asphyxia.—*Effects of the Hot Bath.*—Thirteen animals, as nearly as possible the same age and strength, were drowned, that is, were submerged until respiration had entirely ceased. Seven were placed in the warm bath, on removal from the cold water, and the other six left to themselves. Of the latter, four recovered; of the former, only one. With regard to the morbid appearances in cases of death of the animals put into the hot bath, compared with those of the animals not so treated: "in the animals put into the bath, the lungs were much more congested, more full of blood; they were firmer in substance and specifically heavier than those of the animals not so treated. Both sides of the heart were loaded with blood. In some instances the blood was coagulated in the vessels of the lungs, the systemic veins, and the cavities of the heart. The blood was generally less fluid than in the animals not put into the bath, and coagulated more rapidly when removed from the vessels. In no instance did the bath produce a respiratory effort or any movement whatever on the part of the animal; it seemed, further, to shorten the duration of the heart's action." In those animals which had died after being placed in the hot bath, the lungs were dark and firm, and almost liver-like in appearance. In some instances portions sank in water. These experiments seem quite sufficient to show the injurious influence of the hot bath, both when asphyxia is complete, and when recovery is commencing. "In the former case, the bath momentarily increases the circulation; but respiration being in abeyance, the lungs become loaded with blood, and the left side of the heart distended. In the latter case, respiration being imperfect, engorgement of the lungs takes place, and subsequent arrest of the heart's action." The prolonged use of the hot bath in asphyxia is not only inefficacious but dangerous, and its temporary use is attended with no direct benefit. Probably, by far the most efficacious means of producing respiratory movements is the alternate dashing of hot and cold water on the body. It is better to omit all artificial treatment whatever, than to use the warm bath when respiration is going on freely. (Dr. A. T. H. Waters, p. 371.)

Hooping Cough.—The common clover hay *(Trifolium in fœno)* given as an infusion, ℥ij. to Oj., or as a syrup, has a wonderful effect in relieving hooping cough. The remedy is purely empirical, but it nevertheless cures most cases in a few days. (Mr. M. Foster, p. 47.)

Phthisis.—*Chlorate of Potash and Sugar.*—For some years the author's practice has been to combine the administration of from one to three or four ounces of the chlorate per week, with the use of three or four pounds of sugar during the same period. If the patient has a dislike for sweets, he substitutes sugar of milk. Some

of his cases, from a state of debility and emaciation have grown fat and strong. (Dr. J. Turle, p. 45)

Spasmodic Asthma.—It is a common practice in China, for asthmatics to smoke arsenic, and with the greatest relief to the disease. A very interesting case is related in which a French lady did the same in consequence of hearing the above. She commenced with a quarter of a grain, swallowing the smoke. From being in a state of constant breathlessness and suffering, unable to lie down or make the slightest exertion, she is now able to go about like other persons, and is rarely threatened with an attack oftener than once in three or four months, and that is at once checked by smoking arsenic with a very small quantity of belladonna or stramonium in the dose I have mentioned. She now uses, instead of a cigarette, a small red pipe about five inches long. (Dr. F. G. Julius, p. 47.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Diarrhœa.—Give the following: Compound infusion of gentian, eight ounces; tincture of opium, a drachm to a drachm and a half; nitric acid, twenty minims: one ounce to be taken after every loose stool. (Dr. P. J. Hynes, p. 60.)

Diphtheria.—The best application to the throat which I know of, is Beaufoy's solution of chlorinated soda. A full-bodied camel's-hair brush is the best instrument for applying it, and to ensure its full efficacy the throat should be thoroughly mopped with it as many as three or four times a day. The general treatment I adopt is a supporting diet and a pretty liberal use of alcoholic liquors. The next favourite medicine in such cases is the tincture of sesquichloride of iron in considerable doses. (Dr. W. Budd, p. 56.)

Dysentery.—*Tropical.*—The treatment now usually adopted in the Military hospitals in India is the following. On admission a sinapism is applied to the epigastrium, and ℥xxx. of tincture of opium are given, for the purpose of rendering the stomach more tolerant of the remedy. An hour afterwards ʒj.,—ʒiss. of ipecacuan is given in powder: it is better to give it about half an hour after the tr. opii, but by waiting for a whole hour the medicine is retained longer and produces a more powerful effect. As may be supposed, considerable nausea is the result, but vomiting does not generally come on until at least an hour afterwards, sometimes two hours. During this nausea copious perspiration is produced over the whole body: the pulse becomes more full and soft, and less frequent; the countenance loses the expression indicative of suffering, so marked in acute dysentery; tenesmus and abdominal pain immediately cease, and the patient has no more stools for twelve and sometimes twenty-four hours. (Dr. R. W. Cunningham, p. 57.)

HEMORRHOIDS.—*External.*—When the swelling outside the anus is owing to the rupture of a dilated vein, incise it, and allow the coagulum to escape. When the swelling is owing to enlarged and inflamed portions of skin, excise it with the knife or scissors, and do not depend on the ligature. (Mr. J. Lane, p. 138.)

Internal Hemorrhoids.—In internal piles three modes of treatment may be adopted—excision, the ligature, and nitric acid. Do not employ excision, as the bleeding is sometimes fatal, and you cannot always *get at* the bleeding vessels. The ligature in internal piles, therefore, is much safer, although not so rapidly successful. Mr. Salmon's operation is a kind of compromise between excision and ligature. It consists of a separation of the hemorrhoidal tumour from the subjacent parts, for about the lower three-fourths of its extent, leaving it attached by the remaining upper fourth, which is then included in a ligature. The tumour is drawn down by means of a hook with four prongs, and the division of the lower part of the tumour is made with scissors. These structures are always supplied by vessels which descend *from above*, close beneath the mucous membrane, and the trunks being necessarily included when the upper part of the tumour is tied, hemorrhage is avoided. In nearly all internal piles there is a swollen and everted portion of skin outside the anus. Do not cut away too much of this, else you will produce stricture. Remove only all the irregularities, and all the *pendulous* portions of skin but do not meddle with the circular fold which surrounds the anus. To remove too much skin would certainly end in severe contraction of the anus, but it is not so if you remove *mucous membrane* only. *Nitric acid* is very useful when there is a very *vascular condition of the mucous membrane* of the rectum without large and distinct tumours. (Mr. J. Lane, p. 138.)

HERNIA.—*Radical Cure of.*—The following is the method of operating now practised in most cases of scrotal hernia by Mr. Wood. An incision directed obliquely downwards and outwards is first made through the skin of the scrotum over the middle of the hernial sac, with a small narrow-bladed knife. The edges of the skin are separated from the subjacent fascia by carrying the blade of the knife flatwise under the skin to the extent of three-quarters of an inch from the margin of the incision. The denuded fascia being invaginated by the fore-finger of the right hand into the inguinal canal, carrying the sac before it, the lower border of the internal oblique is felt for, and raised upon the finger. The curved hernia needle is then carried along the finger at its thumb side (*i.e.*, towards the patient's linea alba) and passed through the conjoined tendon from behind forwards till the point of the needle is seen to raise the skin of the abdomen about one and a-half inches above the pubes. The skin of the groin is drawn inwards as far as possi-

ble, before the needle is pushed through it. A piece of stout copper wire silvered, and tapered off at both ends to a flexible degree of thinness is attached by one end to the eye of the needle, and drawn back with it through the scrotal incision, and then detached and left in the wound. The invaginating finger is then carried outwards behind Poupart's ligament, or external pillar of the ring, as high up as possible. The aponeurotic structures are lifted upon the finger, and the needle passed through them on the finger along its opposite border (*i.e.*, that nearest the ilium of the patient.) The point of the needle is carried through the same aperture in the skin of the groin as in the first instance. The other end of the wire is next fixed to the needle, drawn back with it into the scrotum, and also detached. The sac of the hernia, at the scrotal incision, is then pinched up from the cord behind by the thumb and forefinger of the left hand, and the needle passed across it in front of the cord, in the same manner as in passing a needle for the cure of varicocele, but within the limits of the scrotal incision. The inner extremity of the wire (*i.e.*, that which included the conjoined tendon) is then fixed to the eye of the needle, and drawn through the sac across the front of the cord. The two ends of the wire are now drawn down till the loop or bend is close to the upper puncture, the loop being held by the forefinger of the assistant. The ends of the wire are twisted firmly down upon the fascia and sac in the lower puncture, and cut off at a point about five inches distant from the skin. By traction upon the upper loop the twisted ends of the wire are then drawn up into the inguinal canal, invaginating the sac and fascia, and leaving, when drawn tight, about two inches protruding from the lower puncture. The upper loop of wire is next twisted firmly down into the puncture in the skin of the groin until the sides of the canal are drawn firmly together. The lower or cut ends of the wire are then bent into a small hook, and curved upwards, so as to meet the upper or loop end curved downwards to meet it in a corresponding degree. The hook is fixed into the upper loop, a pad of lint inserted between the wire and the skin, and a spica bandage and compress put over all, so as to exercise firm pressure upon the posterior wall of the canal. The spermatic cord is left behind the wire with a small portion of the neck of the sac covering it, this part of the sac being obliterated by the pressure of the wire backwards upon it. (Mr. J. Wood, p. 127.)

New Operation for Radical Cure of.—Having invaginated the scrotum upon the finger, transfix the scrotum at the apex of the invaginated portion with a long, strong, curved needle, fixed firmly in a handle, and armed with a silver wire. The needle must be carried through the internal column and the skin of the abdomen, one end of the wire seized and the needle retracted. Again pass the needle through the external column, and, by gliding the skin upon the needle,

pass the needle out at the same opening in the skin of the abdomen as before; retain the other end of the wire and withdraw the needle completely. The finger must now be withdrawn, and the two ends of the wire drawn tight and twisted, so as to bring the columns into close apposition. Draw the twisted wire firmly outwards, and clip off as close as possible to the skin, so that when the traction is discontinued the ends of the wire are completely concealed by the integuments, which heal over them. The wire remains permanent; it is soon imbedded and isolated in a deposit of lymph. It is essential to success that the loop encircle the columns of the ring near their points of attachment to the pelvis. This operation is very easily and quickly performed, and is very safe and successful. Moreover, the patient is only withdrawn from his ordinary occupations for five or six days at the furthest. (Prof. Chisholm, p. 128.)

Prolapsus Ani.—Sometimes the greatest relief may be obtained by combining the use of nitric acid with subsequent removal of transverse folds of integument from around the margin of the anus. Proceed as follows: Give an injection, get the prolapsed portion well down, and apply the nitric acid to the prolapsed portion. In two or three days repeat the application if necessary. In two or three days more remove from around the anus the thickened folds of skin and mucous membrane. by making a few separate incisions, at *right angles to the orifice, dipping the points of the scissors well inside the sphincter.* If the mucous membrane protrude again after this, apply the nitric acid again after a time, and even a fourth or fifth time, if necessary; but it is better *not* to use the nitric acid after the external incisions have been made, if you can avoid it. By and by use a bougie, else there will be contraction of the anus. This operation will not answer where there is much indurated and thickened membrane protruded, or when conjoined with large hemorrhoidal tumours of a blue colour; but it will answer when the mucous membrane is not indurated, however vascular and voluminous it may be. (Mr. H. Smith, p. 141.)

Rectum.—*Flat Ulcers of.*—These exist just within the anus. Keep the bowels gently open, and apply an ointment composed of a drachm of the nitric oxide of mercury to an ounce of lard. (Mr. W. Coulson, p. 149.)

AFFECTIONS OF THE BONES AND JOINTS, ETC.

Amputations.—The rectangular operation of Mr. Teale has many advantages. When you operate on the leg be careful to avoid wounding the anterior tibial artery at the base of the flap; if you do this you make the flap more liable to slough. "First mark out the anterior flap by carrying the two lateral incisions through the

integuments only, and the transverse incision through all the structures down to the bone. The aponeurosis covering the muscles in front of the leg should next be divided throughout the whole length of the flap, close to the edge of the tibia on one side, and of the fibula on the other. This being done, there is little need for the further use of the knife, as the finger is almost sufficient to detach the mass of muscles upwards from the interosseous ligament, more especially in the situation of the artery, a few touches of the knife being only required laterally to separate the muscles from the bones." The stumps made in this way are very superior and bear pressure much better than in cases of the circular and double-flap operations. This is a point of the greatest importance to the future comfort of the patient, as in many, if not all other kinds of amputation of the leg and thigh the patient has to support himself by an apparatus resting either on the head of the tibia, or the tuberosity of the ischium. (Mr. Teale, p. 75.)

Dislocation of the Shoulder.—To reduce this dislocation the shoulder should be steadied firmly by two stout rollers, one passing over the clavicle and under the chair on which the patient is sat, the other round the chest and under the axilla of the injured side, the ends being secured firmly to the wall or some unyielding substance. The extending force should be applied from the wrist of the injured side; the tendon of the biceps being thus rendered tense, assists in restoring the bone to its place; moreover a much longer leverage is thus obtained. The operator must place his foot on the edge of the chair, and use his knee as the fulcrum. (Dr. N. R. Smith, p. 93.)

Dislocation of the Knee-Joint.—It is a great mistake, when, in cases of inflammation of the knee-joint you see contraction taking place, not to correct it at once. This very flexion is in itself the cause of the necessity for amputation in many cases, and must be most carefully guarded against. (Mr. J. Hilton, p. 81.)

Fracture of Radius.—Remember that nearly all fractures of this bone are near the wrist. When the arm is placed midway between pronation and supination, any pad applied in front, and covered with a splint, will exercise its chief pressure along the centre of the fore-arm, and will not be sufficiently oblique to restore or maintain fully the concavity of the radius. Therefore use a splint which has attached to its radial border a piece of wood, so bevelled that it fills up and fits accurately the natural concavity of the radius. The fracture renders the palmar substance of the radius convex instead of being concave. The surface of the splint is also convex, and covered with tow. The convexities of the splint and radius are mutually brought into apposition. The breadth of the splint at its lower end should not exceed that of the fore-arm at the wrist: it should not project so much internally as to be on a level with the

inner border of the fore-arm. On the radial side it should project beyond the radius, and the bevelled piece for filling up the concavity of the radius should be attached half-an-inch at least internal to that border. By this arrangement the ulnar side of the fore-arm sustains the pressure of the bandage or straps. The reverse is the case on the radial side. (Dr. A. Gordon, p. 97.)

Housemaid's Knee.—Pass through the tumour a thickish thread, which allow to remain in. This sets up inflammatory action, known by a little redness around the entrance of the thread, and the swelling either subsides altogether, or, what is more common, an abscess forms, which is opened, and the cavity becomes obliterated. This treatment is only suitable after all acute symptoms have some time subsided, and best of all to the hard and indurated bursæ. In some cases the walls of the cyst have become so thick and solid that no plan of treatment short of actual removal will prove of any avail. (Mr. F. C. Skey, p. 88.)

Loose Cartilages in the Knee-Joint.—To remove these loose cartilages adopt the following plan. Fix the cartilage firmly at the lower side of the joint, and entrust this to an assistant; next make a subcutaneous separation of the cellular tissue with a long tenotomy knife, two inches in length. From this puncture direct the knife upon the cartilage and freely incise the synovial membrane; then slide the cartilage along the track prepared for it, and fix it at the end of its track with a pad of lint, strips of plaster, and bandage. Apply a straight splint at the back of the limb, and place it at an angle of forty-five degrees. This will keep the parts at perfect rest. (Mr. W. J. Square, p. 83.)

Pirogoff's Operation.—It is a decided improvement in Pirogoff's operation to saw through the calcaneum from below upwards, previously to disarticulating the joint. (Prof. Fergusson, p. 92.)

AFFECTIONS OF THE URINARY ORGANS.

Albuminous Urine.—It is well known that in some specimens nitric acid occasions a cloudiness from the precipitation of uric acid or urates quite independently of albumen. If the test be applied in the following manner it is easy to distinguish between them. Let the test-tube containing the urine be held in an inclined position, and the acid allowed to trickle slowly down its sides; the heavier liquid will sink through the urine, and form a distinct layer at the bottom of the tube. If albumen be present, a cloudy zone will be produced *immediately above and in contact with* the nitric acid; but if no albumen exists in it, but an excessive proportion of urates, a cloudy zone will appear *at the surface of the liquid*, while the lower part, that in contact with the nitric acid, will remain perfectly clear. (Dr. L. Beale, p. 66.)

BLADDER.—*Irritable States of.*—Buchu is a favourite medicine with many practitioners in some affections of the bladder and kidneys, but Mr. Thompson thinks that it is often excelled by the root of the grass called *Triticum repens.* In vesical irritability from affections of the prostate and bladder, calculus, stricture, &c., it is a valuable medicine. Infuse one ounce of the dried and cut stems in one pint of boiling water for an hour. Give from twelve ounces to one pint in the day, in divided doses. (Mr. H. Thompson, p. 177.)

DIABETIC URINE.—*How to ascertain the quantity of Sugar easily.*—Take the specific gravity of the urine before and after fermentation, and from the loss of density occasioned by the conversion of sugar into carbonic acid and alcohol, calculate the amount of sugar destroyed. "The analysis of a diabetic urine by the clinical method is thus performed:—About four ounces of the urine are poured into a twelve-ounce phial, and a lump of German yeast, of the size of a small walnut or so, is added. The phial is then loosely corked, or covered with a slip of glass, and placed on the mantel-piece, or other warm place to ferment. Three or four ounces more of the same urine are likewise put up tightly corked in a *companion phial*, without any yeast, and placed beside the *fermenting phial.* Fermentation soon begins, and proceeds with such rapidity, that in twenty-four hours not only is the process completed, but the froth and scum are dissipated from the surface, and the urine, though still cloudy, is in a fit state for the second step in the process,—namely, taking the densities. It is advisable, however, to remove the phials from the mantel-piece a couple of hours beforehand, so that their contents may cool down to the general temperature of the room. The specific gravity of the two products is then separately observed in the usual way,—by pouring the urine into a cylindrical glass vessel, and dropping in the urinometer. The fermented urine will be found to have suffered a very great diminution of density, ranging from thirty to forty degrees—more or less, according to the amount of sugar destroyed. This diminution of density holds such proportion to the sugar originally present in the urine, that, for each grain of sugar per fluid ounce, there is a fall of one degree of density; or, in other words, *for every degree of density lost, you may count one grain per ounce of sugar in the urine.* The following example will serve to illustrate the proceeding:—

Density of a diabetic urine before fermentation				1040	degrees.
Density after fermentation	...	...	...	1002	...
Density lost by fermentation	...	...	...	38	...

These 38 degrees of 'density lost' indicate that the urine contained exactly 38 grains of sugar per fluid ounce, or 740 grains per imperial pint. In ten minutes—five minutes one morning and five

minutes the next—an accurate quantitative analysis can thus be made. Indeed, the preliminary part of the operation may be left to the patient himself or his attendant. There is no skill required to set aside a few ounces of urine in a twelve-ounce bottle, with a lump of German yeast, and to place beside it a 'companion phial' of the same urine without yeast. The medical attendant need only perform the second part of the operation—the taking of the densities; so that, with five minutes a-day, he may keep an exact register of the diurnal separation of sugar by a diabetic patient." (Dr. W. Roberts, p. 67.)

New Test for Saccharine Urine.—The following test is stated to be of great delicacy and extreme facility of application. Upon a clean slip of tinned iron, place one or two drops of the suspected material, and hold it over a spirit-lamp: the fluid will speedily evaporate, leaving, if the process be arrested at that point, scarcely a trace upon the metallic surface. Continue the application of heat; in a few moments after the dessication is complete, a spot of an inch or so in diameter, over which the drop had spread with the first ebullition, will gradually assume a rich reddish-brown colour, with a brilliant lustre, as if coated with a film of varnish or Japan lacquer. A strong heat produces a darker colour, but the lustre continues till the heat becomes sufficiently intense to decompose the substances. (p. 72.)

Lithotomy.—In young subjects it is necessary to cut well into the membranous part of the urethra. If this precaution be not observed, the prostate is liable to be pushed on the end of the finger into the bladder, thus inverting the order of things. Operators often, when they have reached the groove of the staff in the membranous part of urethra, as urine escapes, think they have reached the bladder. "No such thing: the membranous part of the urethra, somewhat like a syphon, is generally, if not always, filled with urine—the groove of the staff acting as a conductor of the urine. If you do not cut fully onwards into the prostate, so that you can easily pass your finger into the bladder, a complication will assuredly happen. I consider the lateral operation as good as any other: other modes are introduced—I shall abide by the lateral cut. For simplicity of procedure, safety in execution, freedom of action, and ample space of opening—these circumstances all recommend the lateral operation to me before all the other modes, especially in children. I use the scoop in children in preference to forceps; but small forceps may be used on some occasions." (Prof. Fergusson, p. 174.)

Phymosis.—The prepuce being put upon the stretch, divide it directly backwards for about a quarter or half an inch. The glans will be exposed, and the adherent mucous membrane of the fore-skin may be separated. By means of a blunt-pointed pair of scissors, such

portions of the redundant skin and mucous membrane may be cut away, as will allow the penis and glans to remain exposed, and the cut edges of the skin and mucous membrane may be brought together with fine sutures. (Mr. P. C. Price, p. 173.)

Renal Dropsy.—*Succeeding Scarlet Fever.*—When the urine is albuminous after scarlet fever, and the limbs dropsical, give the following draught to a child six years old: Benzoate of ammonia, five grains; spirit of nitrous ether, fifteen minims; syrup of tolu, one drachm; camphor mixture, one ounce; to be taken three times a day, and an occasional purge of the compound jalap powder. (Dr. J. Taylor, p. 11.)

Stricture of the Urethra.—It seems that out of every hundred cases in which external division of stricture has been performed, six have proved fatal. Considering that the disease itself is not mortal, this should deter us from employing external division, except absolutely compelled to it. A stricture, not of traumatic origin, rarely becomes impenetrable to a skilfully-plied bougie or catheter. The most frequent causes of failure are the inopportune employment of the catheter, and impatience in its use. There is not the objection to the simple internal division, recommended by Mr. Thompson, which applies to the external division of Syme. (Mr. J. Gay, p. 152.)

Simple dilatation is safe, efficacious, and *dilatory.* Incision of the stricture by a sheathed knife has long been performed, but fell into disuse, and Mr. Syme substituted his external incision. The danger of internal incision is from hemorrhage, infiltration of urine, or pyæmia: these will be more likely to be caused by many modern methods of internal incision, such as using a blade which is protruded from, and at a considerable angle with, its sheath, after it has passed beyond the stricture, and which is then pressed on the floor of the urethra and slowly and steadily withdrawn outwards. This will often cut the sound as well as the diseased structure, and cause much mischief. Can we therefore have an instrument which will *only* cut the diseased, consolidated, and contracted structure? This can be done either by using a knife so *blunt* that it will only penetrate the part much pressed upon, or one so *sharp* that it will cut by pressure alone, or without any forward or backward motion. For this purpose use Dr. Arnott's instrument described at page 169. (Dr. J. Arnott, p. 166.)

In some of the most obstinate and most discouraging cases, you may pass an instrument and *split up* the stricture at once, with the greatest relief and a fair degree of safety. The instrument consists of two grooved blades fixed in a divided handle, and containing between them a wire welded to their points, and on this

wire a tube (which when introduced between the blades corresponds to the natural capacity of the urethra,) is quickly passed, and thus ruptures or splits the obstruction. The following description in Mr. Holt's own words, will best explain the process: "The permeability of the canal having been once satisfactorily ascertained, the size of the meatus of the urethra is to be gauged by passing into it a sound that will conveniently fit, and the number of the sound so used is to be the number of the tube to be passed: this it is important to ascertain, so that the urethra may not be stretched beyond its natural limits, for while the urethra of one person will admit a No. 14, another will not admit more than No. 9. The dilator having been previously well oiled, is to be introduced with the handle somewhat over the patient's left hip, and by keeping the convex portion gently pressing against the under part of the urethra, the point will glide along the upper portion until it is fairly beyond the triangular ligament, when, by bringing the handle to a right angle with the body, and gradually depressing it—but not so much as in the passage of an ordinary catheter—it will usually slip into the bladder; in fact, the same proceeding is to be adopted as in introducing a lithotrite for the purpose of crushing a calculus. Having reached the bladder, the dilator should be gently rotated, to prove that it is fairly within that viscus, and being thus assured, the surgeon is now to place the point of the tube he had previously selected upon the wire between the blades, and thrust it quickly onwards to the end. The stricture being now fairly split, the dilator should be rotated to still further separate the tops of the rent, and be then withdrawn, a catheter corresponding to the number of the tube being substituted for the purpose of removing the urine. The catheter is now to be taken out, and the patient sent to bed, with directions to take, every four hours, for the first day and night, a mixture containing in each dose two grains of quinine and ten minims of the tincture of opium." (Mr. B. Holt, p. 154.)

Varicocele.—Cases of pendulous testicle, as they present themselves in practice, may be divided into three classes: 1, Those in which the skin and subjacent tissues are relaxed; 2, Those in which the veins alone are enlarged; and 3, Those in which both skin and veins are relaxed and enlarged. The first may be remedied by the removal of a portion of the integuments only; the process of cicatrization braces up the surrounding parts, and the evil is thus remedied. In the second class of cases the obliteration of the veins is requisite, and this may be done by subcutaneous section, without any wound in the skin beyond that which is required to admit the thinnest knife. But in the third class, where the skin is much relaxed, the simple obliteration of the veins will not remedy the pendulous condition of the testes. In such instances it is necessary

to remove a portion of the skin together with the subjacent enlarged veins. The best mode of operating is the following: Having introduced a thin needle between the vas deferens and the enlarged veins, a figure-of-8 ligature is passed over the ends of the needle, and the bunch of veins is compressed. The same proceeding is to be repeated at the lower part of the scrotum. In each case a piece of thin leather is placed under the thread to prevent any pain that the pressure on the skin might occasion. A considerable extent of the skin of the scrotum and of the enlarged veins beneath it are thus included between the two needles, and the circulation in these parts in a great measure obstructed. A thin knife is then introduced about midway between the two needles, and between the vas deferens and the enlarged veins, and directed upwards, so as to divide the integument near the upper needle. A second incision is made to pass downward, so as to divide the skin near the lower needle. A wedge-shaped portion of tissue—skin, enlarged veins, and all—is thus removed, and a very considerable interval left. Some extremities of divided veins, and some cellular tissue, may be removed with scissors, and a clean surface left. Although such an extensive surface is exposed is so vascular a part, yet no hemorrhage follows; this being, in fact, completely controlled by the acupressure of the needles. The needles are now drawn together and maintained in position, and thus approximate the edges of the wound, which may then be dressed by a piece of dry lint, and the patient sent to bed. (Mr. H. Lee, p. 175.)

Pass, by means of a needle, a double silver wire between the vas deferens and the veins, and by the same apertures in the opposite direction, behind all the veins, another wire. By engaging one free end of the wire through the loop of the other one, and doing the same on both sides, the veins may be compressed by traction on the wire. This traction must be kept up constantly by means of an elastic watch-spring, bent in the shape of a horse-shoe. In ten days' time, the wires may be cut off as short as possible, and the skin will heal over the remaining portion, from which no inconvenience is ever felt. (Mr. R. Davies, p. 176.)

AFFECTIONS OF THE SKIN, ETC.

Cicatrices from Burns.—The causes of the unfavourable results so frequently experienced after operations for the cure of cicatrices from burns, is probably owing to too little attention being bestowed upon the subsequent process of extension or stretching. It is probable that where assisted by a cutting operation, and careful and continued extension of the cicatrix is maintained, much more successful results will follow. It is in some cases a good plan to begin with extension, carry it on perseveringly for some time, and then, if neces-

sary, to employ subcutaneous section of the most resisting bands of the cicatrix. Finally, if this fail, the more severe operation of dividing the cicatrix may be resorted to. The best and perhaps the only instrument which will answer the purpose of keeping a cicatrix on the neck fully extended, consists of a shoulder-piece of steel, fitted closely and firmly over the shoulder before being hardened, and retained in its place by two straps passing round the chest. A firm cushion is placed against the lower jaw, and fixed there by two straps passing over the top of the head; the shoulder-piece and the cushion are connected by two steel slips, which can be lengthened at pleasure by means of screws: thus, any amount of extension can be applied at will. This apparatus must be taken off every night, and be re-applied during the day. Some of the most resisting bands may be divided subcutaneously. Where after a fair trial of this for many months, no improvement is perceptible, make an incision both above and below the cicatrix, close to it, but through the healthy skin: the cicatrix may then be dissected from its attachment sufficiently to admit of extension to the desired extent. It must be remembered that much perseverance in the use of extension is necessary to success, but success often follows ultimately. (Dr. J. K. Barton, p. 190.)

Cystic Tumours.—Many cystic tumours are formed by the obliteration of some natural outlet of a gland: of this nature are sebaceous tumours. In these cases look for the obstructed outlet, usually a small black pore, remove the obstruction by pressure or the introduction of a pin or needle; squeeze out the contents of the tumour, and then destroy and cause exfoliation of the cyst. For this purpose a piece of chloride of zinc paste may be passed into the interior. The cyst will usually mortify, and will either come away in small pieces, or it may by a little manipulation be removed by the forceps. (M. A. Courty, p. 184.)

Eczema.—We can always cure eczema, however long it may have continued, though when the disease depends upon internal causes there may be considerable difficulty. In these latter cases remedies given internally may be required, as mercury or arsenic; but in the great bulk of cases external measures only are requisite. When the disease arises from excess of secretion, as in the axilla, or between the buttocks, starch mixed with oxide of zinc is an excellent application. Oxide of zinc (℥j to ℥j of lard), sulphate or acetate of zinc, alum (℥j to ℔j of water), red or white precipitate (6 to 12 grains to ℥ij of lard), are of good service in acute eczema, or in chronic when there is but slight infiltration, and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost indispensable *schmierseife*, by reason of its slightly caustic action, becomes the most preferable remedy. Its

mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar. In many chronic cases, with great infiltration, it may be necessary to employ a stronger form of potash than the *schmierseife* soap, viz., caustic potash, one drachm, dissolved in two drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. The part must be well washed, and cold applications constantly applied. One or two such cauterizations usually suffice. (Prof. Hebra, p. 201.)

Glands.—*External Lymphatic.*—When suppuration takes place, followed by induration, scars, and uneven surfaces, apply a lotion composed of one drachm of iodide of potassium, one drachm of carbonate of potash, one ounce of spirits of wine, and eleven ounces of water. This should be kept constantly applied. Mr. Price substitutes the iodide of ammonium for the iodide of potassium. Sir Astley Cooper used the chloride of ammonium. Sometimes the scar is so disagreeable that it is necessary to destroy it. For this purpose caustics may be used, as chloride of zinc and potassa fusa. Potassa is the best, as the subsequent scar is more shining and smooth. Sometimes the knife is required; the puckered cicatrix may adhere to the subjacent parts, and you may then divide these adhesions subcutaneously by introducing a fine knife at a little distance, and wiping it along, under the skin, so as to accomplish what you want. You must then draw the cicatrix away from the divided bands, so as to prevent adhesion again. (Mr. P. C. Price, p. 178.)

Small-pox.—*Pitting from.*—The best method of preventing the pitting which occurs so frequently in cases of small-pox, is to apply the linimentum aquæ calcis on cotton wool. Pour the liniment on a plate, and dip in it masses of cotton wool, answering to the size and shape of the parts to be covered. Apply the wool so as to completely cover the face and neck, leaving apertures for the eyes, nostrils, and mouth. This dressing may be allowed to remain until convalescence; and if it becomes accidentally detached at any part it should be immediately renewed. (Dr, J. Bell, p. 188.)

Scabies.—Itch may be cured most rapidly by benzine. Every part of the body must be well sponged with it; one or two applications will be sufficient. (Mr. J. J. Godfrey, p. 199.)

VENEREAL AFFECTIONS.

Gonorrhœa.—When the discharge has existed a few days only, the following is the practice usually adopted amongst soldiers by the author. A blister six inches by four is placed very high up towards the anterior and inner aspect of each thigh, and is safely confined in its place by a broad slip of adhesive plaster at each end. For convenience the blisters are generally directed to be applied at night, and no harm has ever come of it, and usually they are well risen by the morning. The patient is then ordered to take every four hours an ounce of the following mixture:—Sulphate of magnesia, two ounces; carbonate of magnesia, four drachms; potassio-tartrate of antimony, two grains; tincture of hyoscyamus, two drachms; peppermint-water, to sixteen ounces. He is placed on spoon diet, with rice pudding for dinner, and a pint of imperial drink should he be thirsty. During the day he is directed to inject now and then a syringeful of cold or lukewarm water, according to the temperature and season of the year. The blistered surfaces to be dressed with lint dipped in castor oil (a favourite dressing with the men). At first there is often a little aggravation of the symptoms, but by the third morning the discharge is much diminished in quantity, the scalding in micturition is less, and the patient feels better. In another day the symptoms have nearly disappeared. The saline purgative must be continued through the case, but in diminished quantities, and the patient must continue to drink freely of mucilaginous drinks; and rest and low diet must be rigidly enforced. Ordinary recent cases are curable in four days by this treatment. Old obstinate cases of gleet seldom fail in being completely relieved by blisters; and in instances where the first application is insufficient, a second set of blisters will prove completely effectual. Occasionally a weak nitrate of silver injection is of service. The complication of swelled testicle in no way invalidates the success of the treatment by blisters. (Mr. H. C. Miles, p. 211.)

As an injection, use three times a day a solution of pernitrate of mercury, made by dissolving half-a-minim of the liquor hydrargyri pernitratis in one ounce of water. (Mr. B. Childs, p. 211.)

Dr. Irvin seldom resorts to any other remedy in the treatment of urethral inflammation than chlorate of potash. His method of using it is as follows:—One drachm of the salt dissolved in eight ounces of water, of which an injection is given every hour for twelve hours, at the end of which the discharge has become changed and diminished, allowing the remedy to be gradually discontinued until

the second or third day, when the disease will be generally found to have ceased. (Dr. Irvin, U.S., p. 216.)

Mix twenty parts of well-washed bismuth (if not well-washed, the acid which remains may give rise to irritation,) in 200 parts of distilled water, causing as much to be taken up as possible. Let this be used as an injection, a local emollient bath being first employed. This is not so useful in chronic as in *acute* cases. (M. Caby, Dr. Mouslon, p. 210.)

Psoriasis.—*Mercury and Arsenic.*—Mercury is a never-failing remedy in cases of syphilitic psoriasis, but has no power at all over cases of ordinary psoriasis. Arsenic is almost a specific in cases of non-syphilitic psoriasis, whilst it is of no use in cases of syphilitic origin. By far the best way of employing mercury in cases of syphilitic psoriasis is the use of calomel vapour baths. Let the patient heat to redness half a common brick, and place this in a common pan, the bottom of which contains a little boiling water. The calomel (20 grains) must be placed in a thin layer upon the surface of the brick, and the whole placed beneath a cane-bottomed chair, on which the patient must sit enveloped in a blanket, drawn tight round the neck, but covering the body loosely. The hot brick has the effect of keeping the water boiling, at the same time as it volatilizes the calomel. (Dr. M'Call Anderson, p. 213.)

AFFECTIONS OF THE EYE AND EAR.

Black Eye.—The discoloration may be got rid of in a few hours by applying the washed and scraped root of the "convallaria," or "Solomon's Seal." A tingling sensation is produced at first. The convallaria is readily found. (Anon., 221.)

Eustachian Catheter.—*How to pass.*—There is only one fixed rule, namely, to keep the catheter on the floor of the nasal cavity; all other rules are liable to numerous exceptions. In general the beak of the catheter is directed downwards, and towards the septum, as the catheter passes through the nasal passage. Sometimes, however, it must be directed upwards; and in others the floor of the nasal cavity passes obliquely outwards and downwards, and is as it were hollowed out; in this case the catheter must be kept far from the septum. If any resistance is met with, rotate the instrument gently, but employ no force. Having passed the catheter through the nose, turn the beak of the instrument downwards, if it has not already in its transit been held in this direction, which in many cases it may be. When the posterior wall of the pharynx has been reached, two ways are open of bringing the catheter into the mouth of the Eustachian tube. Either its beak is turned immediately outwards on the posterior wall of the pharynx, whereby the instru-

ment is brought into Rosenmüller's cavity, therefore behind the projection of the tube, and is now drawn gently, feeling our way, straight forwards over the prominence of the orifice; and so soon as it is felt that it has glided over the latter, it is gently pressed into the mouth of the tube; or the beak of the catheter is drawn, directed downwards, from the posterior wall of the pharynx directly forwards, back to the velum palati (in so doing it is well to press the beak with somewhat greater force still more downwards, in order to be sure of hitting on the velum, and not by more gentle manipulation to run the risk of returning into the nose); having arrived there, the beak is turned outwards and somewhat upwards, and pushed *a little* forwards, whereby it passes into the mouth of the Eustachian tube. The latter operation is often still further facilitated by causing the patient to make the movement of swallowing at the moment when the quarter-turn is made, and the catheter is thus usually involuntarily thrown into the mouth of the tube. (Dr. Voltolini, p. 221.)

Eyelids.—*Adhesions of.*—When the eyelids adhere to any part of the eye-ball, so as to interfere with vision, or to render the movements of the eye-ball difficult, observe the same principle in dividing these adhesions and transplanting healthy tissue as you do on the skin, when you wish to remedy the cicatrices from burns, &c. Dissect the eyelid from the eye-ball, so as to allow the eye to move freely; fill up the raw surface by one or more flaps of conjunctiva from the parts adjacent (only use the conjunctiva for this, and do not include the subconjunctival fascia); and keep the parts together by sutures. (Mr. T. Pridgin Teale, p. 219.)

Presbyopia.—A very rapid increase of presbyopia is one of the premonitory symptoms of glaucoma; but suspicions should be aroused, and we should not fail to examine as to the presence of other premonitory symptoms of glaucoma. (Mr. J. S. Wells, p. 220.)

MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN.

Amenorrhœa.—If local stimulation be required in the uterus to induce the menstrual flow, in cases of amenorrhœa, the best means at our disposal is the introduction of a small galvanic intra-uterine pessary. This instrument consists of a small copper bulb, from the middle of which rises a stem, made half of copper and half of zinc, and two and one-third inches in length. The stem of this instrument must be introduced, so that the upper surface of the bulb is felt to be in apposition with the lips of the cervix. The instrument may be introduced by the fingers alone, but it is so made that a staff may be fitted into the lower end of it, if necessary, to facilitate its introduction. In the majority of cases the instrument

thus introduced will remain of itself *in situ*. After it has been worn for some days, the zinc portion is encrusted with saline matter, and this facilitates its remaining in position; it should not however be allowed to accumulate much. If necessary, a small gutta-percha pessary may be introduced below the intra-uterine pessary, for the purpose of supporting it and preventing its slipping out. (Dr. J. Y. Simpson, p. 358.)

One reason why *electricity* is not more used in the treatment of amenorrhœa, is, that it cannot well be effectually applied externally. In young and unmarried women, however, in whom we are led to suspect a torpid state of the vaso-motor nerves of the ovaries and uterus, and also in such cases where the catamenia have been suppressed in consequence of emotion and cold, electricity externally applied is a most valuable emmenagogue. Faradisation of the skin of the soles of the feet by means of wire brushes, or of the abdominal parietes by means of moistened conductors; and finally, the application of one moistened conductor to the nape of the neck, and of another over the os pubis, are the most effectual methods in cases of this kind. Perseverance is just as necessary with electricity as with most other remedies. (Dr. Althaus, p. 357.)

Atrophy of Children from Dirt-Eating.—Cases now and then occur in which children are ill from some cause apparently unaccountable, until it is discovered that they have a habit of eating dirt, such as earth, cinders, chalk, rags, gravel, lime, in fact anything the child can lay its hands on. The symptoms produced are anæmia, tumid unhealthy abdomen, a downcast and forbidding look, palpitation, and others of a similar class. The use of enemata, along with the internal administration of calomel or grey powder, forms the most suitable treatment for ordinary cases. Dr. Corrigan recommends calomel in repeated doses, followed by castor oil and turpentine. When the offending matters are removed we must endeavour to improve the character of the secretions generally, and for this purpose, the alterative powders mentioned, with the continued use of tepid baths, will be found most efficacious. In the convalescent stage the syrupus ferri iodidi, vinum ferri, or other chalybeate, as indicated. Of course the most careful watching, as regards diet, and to prevent the vicious propensity being indulged in, is indispensable. (Dr. W. Moore, p. 374.)

Cerebral Symptoms in Children.—Whenever, in young children, the tuberculous cachexia is much developed, cerebral disease is liable to come on extremely insidiously. In some cases there are scarcely any symptoms at all for a long time after actual disease has commenced, and in others they are so slight as not to awaken suspicion until too late. At the post-mortem examination, the amount of disease is often very small, and yet it has been sufficient to destroy life. (Dr. C. West, p. 151.)

Diphtheritic Ulcer of the Os Uteri.—Diphtheritic ulcer of the os uteri is a very rare, but at the same time, very intractable disorder. It is mentioned even by but few medical writers. It has at first sight considerable resemblance to syphilitic ulceration. It commences with redness and pain ; and after a short time there is a crop of small, whitish, smooth, and shining patches of different and irregular forms. These patches are extremely adherent to the cervix uteri, and even friction with lint fails to remove them. At the end of a few days, however, they fall off spontaneously, leaving an ulcer. This ulcer, by some recent French writers of eminence, is said to heal with the greatest facility; this, however, arises from their confounding an aphthous with a diphtheritic ulcer. There is, before the diphtheritic patch has fallen off, a certain slightly rough and dry sensation communicated to the finger, contrasting forcibly with the otherwise polished surface of the os ; this is very peculiar, if not diagnostic. With the speculum the os is seen to gape, and its sides to be deeply ulcerated, the surface of the sore being coated with a whitish membrane. Here and there the membrane is perhaps removed, and a raw vascular bleeding surface uncovered. This disease is most commonly met with at or about the close of the functional life of the uterus. In the treatment of this affection carefully avoid the use of nitrate of silver ; it invariably aggravates the symptoms much. Neither the tincture of iodine nor the potassa cum calce produce this effect, and these form our best means of curing this most tedious complaint. The actual cautery applied by the electric wire is very useful. The wire must be maintained at a white heat by a powerful Grove's battery, and tightly drawn over the ulcer. It is useless applying any of the weaker caustics. But an exclusively local treatment is by no means successful. There is a deep-lying fault in the patient's constitution, and the effort must be made to rectify that co-existingly with the absolutely indispensable use of local means. Iron, quinine, and chlorate of potash present themselves as the remedies for the restoration of the general health ; and an occasional course of cod-liver oil will be useful, if the digestive powers can be got up to the point of its easy assimilation. (Mr. R. Ellis, p. 329.)

Dysmenorrhœa.—*From Mechanical Obstruction.*—The canal may be first dilated by a small plug, and then some cutting-instrument must be used. For this purpose Dr. Coghlan has invented an instrument about eight and a half inches long, including the handle, and slightly curved. The point is blunt; and, at the distance of a quarter of an inch from the extremity, the instrument swells out into two cutting blades, about three-eighths of an inch wide—these again subsiding into the stem. The advantage of the instrument is, that, guided by the probe point, we are sure with ordinary care to have our incision right into the uterine cavity, and perfectly central, and by using instruments of different widths, we

can have our incision of the exact extent we desire. Having made the incision, take a small leaden tube, made by rolling a piece of sheet-lead, pass it into the cervix, and by means of a pair of long thin dressing forceps introduced into its interior, dilate it to the full extent of the incision. (Dr. J. Coghlan, p. 364).

First Stage of Labour.—When we know from previous experience that a patient will probably have a tedious and protracted first stage of labour from non-dilatation of the os uteri, it is a good plan to give small doses of belladonna for a fortnight before the termination of gestation. An eighth of a grain of extract may be given three times a day combined with a very small amount of antimony. (Dr. B. F. Barker, p. 291).

Forceps.—*Application of.*—When the head has entered the pelvis so that an ear can be felt easily, and labour is going on briskly, the child becomes in danger if delayed too long, and if there be some impediment: therefore use the forceps. If you do not know the exact position of the head, feel for an ear, fix the forceps over the ears and simply make traction, until the head has somewhat advanced. Then, if possible, gently turn the head round at the same time that traction is being applied, so as to bring the face into the hollow of the sacrum. Sometimes you can best do this by making it turn towards the right side, sometimes towards the left. First, always try to turn it towards the right if the face be towards that side, opposite the symphysis pubis, or if even slightly to the left, as the rectum on the left side diminishes the capacity of the pelvis and offers an obstacle to the free version of the head in in that direction. When the face is towards the left acetabulum, or more decidedly still to the left, first try to turn the face to the left into the hollow of the sacrum, but should you fail, try to turn the face quite round to the right, so as to reach the hollow of the sacrum in this circuitous way. (Dr. G. Hamilton, p. 266.)

There are some cases especially liable to happen in primiparæ, where, from the duration of the labour and size of the fœtal head, the latter becomes much elongated, so much so that the ordinary forceps is quite inapplicable, the blades not being long enough and too sharply curved. For such cases an instrument should be used the blade of which is 8 inches long (instead of 6½ or 7 inches), the curve that of a circle of 14 inches (instead of 10 or 11 inches), and identical in other respects with the ordinary short forceps. (Dr. G. Hewitt, p. 269.)

It is not a fair representation of the application of the forceps to show it as embracing the head with perfect symmetry. The grasp is oblique, one blade lying on the right brow or temple, the other on the occiput, or behind the line of the transverse diameter. This

arises from the long forceps being introduced in the transverse diameter of the pelvis, and not with reference to the child's head. This is an important rule to follow. In some cases, with the head low in the pelvis, it may be possible to apply the short forceps with a blade on either parietal bone; but it is a bad practice. The pelvis should be the guide to the position of the forceps, not the head. (Dr. R. Barnes, p. 271.)

Hysteria.—There are cases in which an hysterical tendency is evidenced less by the simulation of disease which does not exist, than by the exaggeration of the symptoms of some affection which has an actual existence. In these cases, it is the most successful plan not to treat the nervous affection, but to continue to direct the treatment principally against the original affection. Thus, in a case of syphilis, with hysterical contraction of the elbow-joint, treatment directed so as to cure or relieve the syphilis, cured the hysteria. (Mr. W. Coulson, p. 350.)

Ovariotomy.—In operating for ovariotomy, do not close the wound perfectly around the peduncle, so that any discharge may find vent. Sometimes a little peritonitis sets in, and the serum formed is highly poisonous, so much so, that a most minute scratch of the finger touched with it causes severe symptoms. (Mr. T. S. Wells, p. 348.)

Perineum.—*Support of in Labour.*—The chief uses in supporting the perineum are, to relieve the irritation caused by the descent of the child, to diminish congestion of the part, and to counteract the too powerful contractions of the womb, when the perineum is not ready for the head to pass. It is doubtful whether it does any good to press the head forwards towards the pubic arch. Forcing the head over a perineum unprepared, is just as likely to cause laceration as drawing it back, or leaving it to itself unsupported. (Dr. E. W. Murphy, p. 260.)

Laceration of the Perineum.—Why does laceration of the perineum occur in one case and not in another? One great reason for this diversity is the position and length of the perineum, and its relation to the pubes and coccyx. The pubic margin of the perineum is nearer the symphysis pubis in one female than in another; and upon this depends whether it is out of the axis of the outlet or not. In one case it is scarcely carried forward at all by the presenting part of the child's head, whilst in another it is carried forwards by the head, and distended from three to five inches; in the latter cases laceration can hardly be prevented. Much may be done by guiding the head forward in the direction of the abdomen of the mother; if we do not prevent it, we may at any rate greatly lessen the rent. (Dr. J. Gray, p. 259.)

Operation for Lacerated Perineum.—Use the needle suture of Dr. Aveling. made by Messrs. Cocker Brothers, Sheffield, and sold by Mr. Aitken, cutler, York. Place the ruptured parts together and secure them with two needle sutures; bend the wire a little before introducing the needle point; pass each end through a smooth flat splint of wood, and fix them by means of pierced duck shot. Do not approximate the parts too tightly, and chiefly attend to approximating the deep parts rather than the superficial. The parts can be approximated more tightly in a short time, if necessary. (Dr. T. Skinner, p. 262.)

It is very essential to success that a sufficient quantity of the mucous membrane be removed from the vagina, to secure its more perfect contraction. There is a firmer and more secure apposition of the parts when silver hare-lip pins are used instead of the ordinary quilled suture. (Dr. D. L. Roberts, p. 320.)

Premature Labour.—To bring on premature labour by injecting water into the uterus is effectual, chiefly in proportion as it separates the membranes from the uterus, but it is a sloppy and often disagreeable method, although generally safe enough. Never inject air or carbonic acid gas, as the open veins may take in the air and cause serious mischief, and even death. One of the best ways is to introduce a flexible male catheter, to separate a portion of the membrane at some little distance up the inside of the womb, taking care as much as possible to avoid the situation of the placenta; then withdraw the stilet and leave the flexible portion of the catheter in the cavity till labour comes on. Labour will commence in about twelve hours, but if the instrument perforate the membranes the labour will perhaps be retarded for twenty or thirty hours. (Dr. J. Y. Simpson, p. 227.)

The use of *caoutchouc dilators* for inducing premature labour is spoken very highly of. A caoutchouc bag is introduced partially within the os, and then filled with water; the pressure so produced acts uniformly and gently. As a means of overcoming rigidity, it is superior in efficiency to bleeding, antimony, or stretching by the hand. (Dr. R. Barnes, p. 231.)

Statistics show that a much greater number of children are saved when premature labour is induced by separation of the membranes by means of injecting tepid water, than when the membranes are punctured. Thus 88 per cent. were saved by Dr. Hamilton, who separated the membranes but did not puncture them, and only 45 per cent. by Dr. Merriman, and still less by Dr. Lee, who punctured the membranes. (Dr. W. O. Priestley, p. 232.)

The plan usually pursued by Dr. Davis when desirous of inducing premature labour, is to introduce a sponge-tent within the os, for the purpose of dilating it sufficiently to admit the finger. Sometimes it is unnecessary to use the tent. Introduce the finger slightly within the os, and pass it round inside so as to dislodge the mucous plug and separate the membranes; subsequently, a piece of soft sponge previously oiled, and as large as can be passed without much difficulty, must be introduced. This plan will answer in most cases. (Dr. H. Davis, p. 233.)

Placenta Prævia.—Contraction of the womb is the condition upon which the arrest of flooding depends, and the great object of all interference in the management of cases of placenta prævia is, to secure this saving contraction of the uterine muscles. In ordinary cases of placenta prævia it may suffice, frequently, to simply detach with one finger passed through the cervix all that portion of the placenta which adheres to the cervical zone of the uterus. It must not, however, be supposed that the separation of the placenta, either partially or wholly from the uterus, can of itself stop the flooding. The hemorrhage ceases because the muscular fibres of the part become more contracted, and are able to close the open mouths of the vessels. If this proceeding does not suffice, tap the amniotic sac, and drain off the liquor amnii. If the uterus still remain flaccid, and hemorrhage continue, or, where the patient is much exhausted, so that effective muscular contraction cannot be roused, artificial aid is called for to accelerate delivery. If the cervix be freely expanded, the child may be immediately turned by two fingers of the left hand passed through the cervix, tilting the head forwards over the pubes, the other hand externally pushing the breech backwards and downwards until a knee or foot is reached. But if the os is undilated and rigid, we must dilate by the caoutchouc dilator. A suitable one is introduced into the os and cervix, and is then distended with air or water, so as to put the cervix gently on the stretch. Besides acting as a dilator, this elastic sac presses on the mouths of the bleeding vessels and arrests the hemorrhage. Sometimes this measure suffices, until the natural powers terminate the case favourably without further help; if not, we may turn and deliver when the size of the os admits; and the hemorrhage being stopped we can wait for this. Sometimes the placenta is found attached only by a small portion, it is then better to remove it entirely. (Dr. R. Barnes, p. 241.)

The old practice in cases of placenta prævia, and with many men even yet, the usual mode of practice is, to wait till the os is sufficiently dilated and then to turn the child. The mortality when this practice is pursued alone is very considerable, being to the mother one in every three cases. The mortality is very much

reduced (to one in forty-four) if, as early as possible in the case the finger be introduced in the os, and the placenta separated as far as it can be reached If the detachment is completely and properly effected, it will rarely fail to stop the hemorrhage. This mode of practice was first suggested and practised by Mr. Kinder Wood, of the Manchester Lying-in Hospital, and was subsequently taken up and advocated by Prof. Simpson, of Edinburgh. It is supposed by many that the separation of the placenta is certainly fatal to the child, whereas only one in five are born dead, whilst when the hemorrhage is allowed to proceed, at least half the children are born dead. (Dr. C. Clay, p. 248.)

Urethral Irritation in Females.—There are cases of urethral irritation in females, characterised by a constant desire to void the urine, yet each attempt, instead of bringing relief, only aggravating the sufferings. The urine is not more acid than usual; it is clouded with mucus. The urethral mucous membrane appears very red and congested, and on the passage of a catheter is found to be exquisitely tender. The bladder itself does not seem to be affected. The best treatment of these cases is to give constant small doses of liquor potassæ, well diluted with water. Also, every day, after washing out the bladder with warm water, to inject a syringeful of solution of nitrate of silver (five grains to the ounce), and allow it to come away of itself. Rest in the recumbent posture must be enjoined. (Mr. J. Hunter, p. 350.)

Uterine Hemorrhage.—One of the ordinary vulcanized india-rubber balls may have attached to it a small brass tube, in the way described at page 299, so as to form a pessary, which will either act as a pessary or to restrain hemorrhage. The simple admission of air through the brass tube is sufficient to dilate the ball, which may be introduced into the vagina in a state of collapsion. (Dr. G. Hamilton, p. 299.)

Vesico-Vaginal Fistula.—Two or three things are highly important in all these operations. Do not cut away any of the mucous membrane of the bladder when paring the edges of the fistula. Do not run the needle through any portion of the mucous membrane, as, if you do, this will probably be the track of a new fistula; hence carry the sutures *close to* but not *through* the vesical mucous membrane. In order to get as broad a surface as possible for adhesion, you may pare the edges in an oblique direction, that is, *bevel* them. Always use metallic sutures. (Mr. T. S. Wells, p. 306.)

Split the margin of the fistula all round, that is separate the edges, but do not pare any away. This splitting of the parts gives a larger raw surface when properly managed than simply paring the edges. Either wire or thread sutures will answer—good thread

is better than iron wire. Each ligature consists of a double thread; and when all are passed through the flaps, a piece of vulcanized india-rubber cord is run through the looped extremities, which are successively pulled tight upon it, and the free ends are then tied over a similar piece of cord at the opposite side of the fissure; the sutures are not to be tightly drawn, as, if the raw edges are tightly drawn together they will *puff up* between the india-rubber quills, and the flaps will slough from being too tightly confined. The advantages of this operation are: that it gives a more extensive raw surface to be united, as the split surfaces by gentle approximation open their mouths, as it were, to be united; this raw surface is obtained without any loss of structure; the ridges which rise up as the split parts are approximated are an advantage, preventing the water escaping and coming in contact with the wound; the operation is simple, and requires no complex armamentarium. Use strong brass divaricators, or even a few common iron spoons, properly bent, will act as divaricators, and a good sharp scalpel will do the work well enough. (Mr. M. H. Collis, p. 309.)

MISCELLANEA.

Alcoholic Stimulants in Disease.—These are *stimulants* and *tonics, i.e., medicines*, and not *food*. It is a grave error to give them too indiscriminately in the early stages of acute disease. When required, give them in very moderate quantities along with food, except to patients who have been largely dependent upon them before the illness. In short, Dr. Todd's views are in many respects incorrect, and not to be followed. (Dr. W. T. Gairdner, p. 383.)

American Hellebore.—*(Veratrum Viride.)*—This drug possesses a great power of calming nervous excitement, and reducing the force and frequency of the heart's action. These two effects, therefore, are invaluable in a medicine. In sthenic diseases it acts like the lancet, without reducing vital power; it is more prompt and certain than digitalis, and yet is not cumulative; it is better than antimony both as a sedative, expectorant, and diaphoretic. The best preparation is Dr. Norwood's tincture, to be obtained from America. If the British practitioner procure this preparation, let him begin with half the dose recommended by Dr. Norwood. Give three or four drops every two hours till the pulse comes down, increasing the dose if necessary. When emesis is produced the pulse comes down sufficiently, the skin softens and profuse perspiration breaks out. In pneumonia, pleuritis, acute rheumatism, croup, iritis, and other inflammatory diseases when the pulse is strong, and in which cases you would give antimony, ipecacuanha, aconite, and

such like sedatives, you may more safely and efficaciously substitute the American hellebore. (Dr. Otterson, p. 392.)

Essence of Beef.—To prepare this put the meat to be used, first chopped up, into an earthen jar, closed at the top, and the edges of the cover sealed up with flour paste. This jar may be placed in the oven for an hour, or it may be placed in boiling water for that period; after which time its contents will be found to have separated into three portions, fat, solid fibre, and liquid essence. The last is easily strained off, and the fat separated by blotting paper. It is, when nicely made, a clear amber liquid, of an aromatic meaty smell and taste. About one-fourth of its weight is obtained from ordinary meat. The object of this preparation is, to administer the purely stimulating elements of meat in a separate form. It may be given in cases of weakness and exhaustion after hemorrhage, or indeed from any cause. It may be given as a substitute for brandy in cases of mental depression, grief, and shock. One tablespoonful taken early in the forenoon, with or without a little bit of crust and a mouthful of wine, "gives the brain a something that relieves it from nervousness and vapours." In many cases of dyspepsia it is a capital remedy. In a few words, it is useful whenever nutriment without bulk is required. (Dr. Druitt, p. 421.)

Galvano-Cauterization.—In comparing the galvanic cautery with other escharotics we must take into consideration that we cannot substitute it for any of the milder caustics, which act rather as astringents. Again it differs from more powerful caustics such as chloride of zinc, Vienna paste, &c., in its action being limited to the part to which it is applied. This property may be advantageous or the contrary according to circumstances, thus, if we wished to effect any considerable destruction of tissue, as in the treatment of cancer, the use of the galvano-caustic would be contra-indicated. As to convenience of application, the galvanic cautery undoubtedly possesses an advantage over all other escharotics, and more especially over the actual cautery, in that it may be brought into contact with the parts in a perfectly inert condition, a property of great importance when the part affected is situated at the bottom of any of the natural cavities or canals. (Dr. T. J. Walker, p. 395.)

Poisoning by Strychnia.—Get the patient under the influence of chloroform, and if the spasms return repeat the process again and again for hours if necessary—occasionally giving a dose of opium, compound spirit of ammonia, and camphor. (Mr. W. Travers, 'Lancet,' October 12, 1861, p. 347.)

A Commentary on Midwifery,

AND THE

DISEASES OF WOMEN AND CHILDREN,

FOR THE LAST HALF-YEAR.

The Medical writings of the British Isles and America have contained an abundance of most interesting matter within the last six months, which we have endeavoured to *winnow* with as much care as possible. In the *Synopsis of Medicine*, at the commencement of the volume, the reader will find most of the valuable facts which bear on the treatment of Diseases published during the past half-year; but, in the following Commentary, we have dwelt more at large on some of the more important questions relating to Midwifery and the Diseases of Women and Children, which the reader will find in still more detail in the body of the work.

The various methods of inducing Premature Labour are still the subject of discussion. We have never yet failed to bring on labour by simply introducing a male silver catheter, and after gently moving it round the inside of the os uteri so as to detach some portion of the membrane, quietly pushing it through the amnion and evacuating the liquor amnii. Perhaps Dr. Simpson's plan is better, though we think that the amnion is more frequently ruptured than is suspected in his operation, and will then only just be the process which we generally adopt. He introduces a flexible male catheter with its stilet, and after withdrawing the stilet, allows the flexible portion to remain in the womb till labour commences, which it almost invariably does in about twelve hours; whereas, if the amnion be ruptured the process is delayed for other ten or twelve hours. Both ways are safe enough generally to the mother, and we have seldom or never found the child injured, unless the pains became incessant and *uninterrupted*, in the same way as by giving a too powerful dose of ergot of rye, whilst the downward progress of the child was arrested by an unyielding os uteri. There must be one evil however in Dr. Simpson's method, which is not alluded to. We all know that in withdrawing the wire stilet from a male flexible catheter, it cannot be done without pulling down the point very considerably. May not this process sometimes be injurious in a thin womb, that is, supposing we introduce the instrument in its usual catheter form? If it be introduced nearly straight, this objection does not hold. We think, however, that this ought to be remembered,

and if you introduce this flexible catheter with its stilet in it, you had better do it with the instrument *as straight as you can well introduce it.*

It seems generally to be acknowledged that it is better to have a dilated os uteri before puncturing or separating the membranes, when we can easily accomplish this dilatation. There are some cases where we cannot do so readily. Few cases, however, will resist the Dilators of the Os Uteri which are now being used. Some practitioners puncture the membranes whenever they can reach them with the finger, the catheter, or any other instrument; some tap the membranes as remotely from the os as possible; some separate the membranes simply from the os and cervix with the finger; some place a flexible catheter in the womb and leave it there; and some use warm or cold water injections, either upon the os or within the womb itself. All these methods assume that it is not well to excite labour pains till the parts are dilated enough to admit the easy expulsion of the child, the same as in natural labour, and so far they are praiseworthy. A safe method of dilating the os is that recommended by Dr. Barnes, who introduces a Caoutchouc Dilator into the orifice, and gradually distends it with water. Dr. Keiller some time ago recommended the same thing, but used air instead of water; and dilating instruments introduced one over the other, as by Mr. Wakley's instruments for stricture, were also some time ago recommended; but we have much more power and command over water than over air, and Dr. Barnes's instrument seems to us excellent. You may introduce it empty just into the os and cervix, and then forcibly but gradually dilate with water. When the parts are ready, you may safely puncture the amnion or not, as you see necessary. It is certainly better to dilate the parts with a soft water-bag resembling the process of nature herself before encouraging the uterus to contract forcibly. We think this same dilating process might be applied to some of the rigid os uteri cases in natural labour; it would save the poor woman endless hours of misery in some cases, and even the life of the child. Instances occur in which the os will not be dilated—it is too rigid; in these cases the os may be incised with safety. Some of our best writers recommend this; and we have occasionally met with cases which have ended favourably where the rigid and unyielding os has been lacerated by the uterine pains alone forcing the head through, so that these parts will bear greater ill-usage, in cases of necessity, than we have hitherto suspected. We must acknowledge, however, that for a long period in our own practice, we have never found any ill effects from simply puncturing the membranes and leaving the rest of the case to nature. A more simple, and almost equally successful practice, is to inject tepid water into the cervix uteri by means of a gum elastic catheter and a Higginson's syringe, as used by Dr. Priestley. Labour will generally come on in twelve hours. In reading the discussions on this subject we are struck with what we consider the

erroneous explanation of Dr. Hall Davis, respecting the cause of asphyxia of the child, in such cases. This talented physician says that the child is asphyxiated *from pressure exerted on its body* by the womb in the absence of the liquor amnii. This we think an error. It is asphyxiated by the long-continued and uninterrupted action of the fibres of the womb so compressing the vessels of the womb itself that the circulation of the mother is no longer of any use to the child, and the placenta might as well have been separated. This is clear enough. It is an explanation which we were the first to give of this interesting fact nearly twenty years ago, and which we have had repeatedly confirmed since.

One of the most interesting and important questions to decide at the bedside of a woman in labour, when the Placenta is presenting and Flooding exists to a dangerous degree, is how to stop this hemorrhage, and whether to remove as much of the placenta from the os uteri as the finger can reach, or to deliver by turning. A great deal depends on circumstances in such a case, but as a general rule, we should say that when the os uteri is sufficiently dilated to admit the hand, you had better turn the child at once and deliver. This is an easy process at this stage of labour, the hemorrhage is arrested with certainty, and the life of the child is saved; so that all the results which are required are accomplished with comparative safety and certainty—very little is left to chance. But it often happens that the os uteri is not enough dilated to admit the hand, and is not even capable of being easily dilated artificially, the parts are too rigid. What is to be done now?—the woman is fearfully flooding. If you pursue the usual treatment, you will now rupture the membranes and most effectually plug the vagina. The os uteri will then gradually dilate, whilst the plug prevents all hemorrhage externally, although this may go on internally till the coagulum arrives at or a little above the level of the bleeding orifices when, we maintain, the flooding will be again arrested and give time for the more complete dilatation of the os, so as to admit of the hand to turn. This process likewise does not sacrifice the life of the child, and is generally safe to the mother, if done adroitly. But another and easier process has often been adopted of late years, and recommended by some of our best practitioners, viz., separating as much of the placenta from the internal os uteri as can be reached by one or two fingers, or by a small portion of the hand. Of course it will be safest for the child to separate as small a portion of the placenta as we can, as the total separation would almost certainly take its life if the labour were at all prolonged: so much however must be detached as will arrest the bleeding. Dr. Barnes is one who has most earnestly recommended the practice. He insists however on one thing which is necessary in order to make the practice successful, viz., *contraction of the womb*. We have therefore especially two things to do in this new practice—to detach the placenta partially, and induce contraction of the womb.

To separate the placenta simply will not be sufficient, we must also excite the contraction of the womb, or the operation will fail. To do this we should not be satisfied with simply giving a dose of secale, but we should first rupture the membranes if we can reach them, and then effectually plug the vagina, as in the old practice. The child's head will then soon be pressed forcibly upon the separated placenta, so as to prevent bleeding internally, and the plug in the vagina, assisted probably by the partially-extruded placenta, will prevent flooding downwards, so that the danger will be completely averted and the child delivered naturally. It is clear that contraction of the womb is necessary to stop the flooding, as the head would remain at some distance above the bleeding orifices and would not thus act as a compress unless forced down by the contraction of the womb: and therefore we think that it is not sufficiently insisted upon to rupture the membranes and plug the vagina effectually, as well as to give a dose of secale. To rupture the membranes may be generally advised, but it is often omitted to advise also to apply the plug. When the os is obstinately rigid, Dr. Barnes recommends the introduction of a caoutchouc dilator, in the form of a bag, which is slipped empty into the part and dilated with air or water. We think, however, that nature herself is the best dilator, and when we have effectually plugged the vagina we may safely wait for the natural dilatation, as we can almost *defy* the bleeding from proceeding to excess after the vagina has been completely plugged with rags. Occasionally we think the fingers of the practitioner may be usefully employed to assist in the dilatation, the plug however to be again used as soon as the dilating operation is done with, if the hemorrhage continue. In cases, then, of Placental Presentation, when the os uteri is too rigid to admit the hand, and when flooding is going on to excess, we would recommend the following practice:—1st. Introduce one or two fingers and separate as much of the placenta as may be around or near the os. 2nd. Rupture the membranes, if possible, either with the finger or the catheter. 3rd. If flooding keep off a little, take this opportunity of artificially dilating the os uteri, either with the fingers or with Dr. Barnes' dilator. 4th. Take a long bit of soft rag, a foot or more long, grease it well with oil or lard, and push it well up to the os, keeping hold of the last portion with the left hand: with the right hand push up the vagina smaller bits of rag till the passage is completely plugged. The left hand has still hold of the long bit first introduced, which should now be bent backwards over the perineum. The object of this is, that at any moment, without introducing the fingers into the vagina, the long bit can be withdrawn, and with it all the rest come away. The woman may want to make water in the night and cannot bear to wait for the practitioner, she can therefore withdraw the plug herself, under certain restrictions, which she could not do if used as commonly directed. Moreover, the practitioner, in some measure, can keep the fingers of his right hand clear

of the offensive discharge by using the left. 5th. If the os begin to lose its rigidity (but not before, for fear of injuring the child,) give a good dose of secale cornutum, and do not trust to ready-made preparations, but make the decoction yourself in a tea-cup, on the top rib of the fire-grate. We are aware that some practitioners may advise the secale to be given as soon as the membranes are ruptured; but we would caution such to observe the rule which we laid down above twenty years ago, viz., not to cause *incessant* and *uninterrupted* contraction of the womb by secale with a rigid os uteri. We have often been amused to read the opinions of some of our best practitioners as to the cause of the child often being born dead after a powerful dose of secale. The reason is simple enough: the child is supplied with oxygen from the blood of the mother; but, if the uterus be kept in a state of incessant and uninterrupted contraction by secale, little or no blood can circulate in the womb, and hence the child is simply asphyxiated; and for the same reason, hemorrhage is often materially checked, simply by these incessant contractions, without any other artificial assistance.

In the discussion on Dr. Uvedale West's paper on the Ergot of Rye, at the Obstetrical Society, the opinions expressed both by Dr. Tyler Smith and Dr. Hall Davis are not sufficiently explicit on the action of this medicine. Dr. Tyler Smith asserts that "it can never be given safely, except under circumstances in which the forceps are applicable." This means that the child is low down, and the uterus is unable to complete the labour without help. This is a good rule as far as it goes, but it would in some cases, be dangerous. When the womb has ceased to act energetically, it would require a very great dose of ergot to re-commence the action; it is better, therefore, to give the medicine as soon as the power of the womb is felt to flag, and when there is no obstruction to prevent the descent of the child. If the uterus be stimulated to incessant action without moving the child downwards, asphyxia will almost certainly be produced; on the other hand, if the os uteri be well dilated, the parts relaxed, and such a moderate dose of ergot be given as will only moderately stimulate the womb, and allow intervals of rest, there is little or no danger to the child. We have watched its effects in hundreds, we may almost venture to say in a thousand cases, and never knew the child asphyxiated when there were *intervals* in the parturient action; but when the womb had not time given to it to have its blood replenished, the danger to the child is great. Dr. Hall Davis's opinion that *pressure on the body of the child* may assist or even cause its death, seems to us to be incorrect: the child will bear the most violent compression that the womb can exert, without danger, provided this pressure be not incessant; and we maintain that it is on the principles we have already laid down that the most satisfactory explanation can be given.

It is almost unfair in modern writers not to give due credit to the

person who seems to have first recommended and carried out the practice of Separating the Placenta with the finger from the os uteri in cases of placental presentation. So long ago as 1822, Mr. Kinder Wood, of Manchester, first noticed that in cases of this kind when the placenta spontaneously separated itself, the flooding ceased. He therefore, imitated nature in this process, and whenever he could do so, he introduced his finger and wiped it round the internal os uteri, separating the placenta from the part; this generally stopped the flooding. Dr. Clay, who has strenuously stood up for the rights of his able friend and late master, does not, however, lay so much emphasis on some other points mentioned in the previous part of this article, viz., rupturing the membranes and plugging the vagina; but we think it very likely that these points were not forgotten by Mr. Wood, and would certainly not be omitted by such a man as Dr. Clay. The main point is the separation of the placenta with the finger when the contracted os uteri would admit of no further manipulation, and this practice seems due to Mr. Kinder Wood.

The greatest *bore* to both patient and medical attendant, in a case of labour, is the long time taken up in the Dilatation of a Rigid Os Uteri in a first labour. He would be a benefactor to both women and doctors who discovered a safe method of shortening the process. Sometime ago we pointed out that chloroform had an excellent effect in accomplishing this object, and we continue to think very highly of it, as directed to be used by us in a former volume. Dr. B. Fordyce Barker suggests that the use of Belladonna for two or three weeks previous to labour will so relax the parts, that the first stage of labour will be shortened; but we should be afraid that belladonna would relax also the whole uterus, and thus weaken its efforts during labour. The effects of chloroform are more evanescent. Chloroform certainly for a short time checks the uterine contractions, but its effects soon pass off, and these contractions are soon resumed, so that you may relax the os uteri beautifully; and when you have accomplished your purpose you can leave it off, and allow the womb to recommence its efforts. But we suspect that you cannot command the belladonna, which has been given for weeks, to cease its relaxing powers just when you like: these effects will be continued all through the labour more or less. Dr. Barker's treatment, however, is interesting. He watched its effects in 147 cases, commencing the belladonna a week or two before labour with a minimum dose, and gradually increasing it till the throat became dry, and slight giddiness and dimness of sight came on. He generally began with a quarter of a grain of the extract of belladonna three times a day, increasing it till its effects became evident.

What is to be done in a case of labour, when the Head makes no progress, and yet there seems to be not sufficient contraction of the parts to account for the difficulty? You feel the head, you feel that there is room enough, and the pains are strong enough, and yet no progress is

made. In an interesting case of Mr. Jardine Murray, of Brighton, such an occurrence took place. On examining carefully, it was found that the elbow was coming down with the head, and the hand was thrown round to the back of the child's head, so that the elbow pressing on some part of the brim was the cause of the obstruction. This case is certainly rare. Mr. Murray proceeded to deliver by turning, but Dr. Eastlake, though in the most friendly tone, dissents from this practice, and advocates that *when the child is dead* the delivery should be accomplished by craniotomy; and when alive, by the forceps, according to the Dublin practice. This becomes an interesting question, and we humbly maintain that it will easily be settled by proceeding in the following way:—It is almost certain that in all such cases the liquor amnii will have been evacuated before the presentation and difficulty have been ascertained, because the truth cannot be properly ascertained till the hand has been carried a considerable distance into the uterus. It is also certain that here we have not an ordinary case of *impaction* low down in the pelvic cavity, but a simple obstruction at the *brim*, with plenty of room. There will consequently be comparatively but little difficulty in introducing the hand and turning, during the state of anæsthesia from chloroform. This was the opinion of Mr. Murray, and we agree with him entirely. Dr. Eastlake thinks that as the child was dead craniotomy might have been preferable, but we differ from him. Craniotomy, when the child's head was at the brim and even above it, would have been quite as dangerous, or more so, to the mother, as skilfully turning the child, and moreover might not have dislodged the elbow completely from its position. The best practice seems to us, therefore, to take the opportunity, while the hand is in the womb ascertaining the difficulty, to turn the child if possible, and if this cannot be done, then it will be time enough to deliver either by the forceps or by craniotomy. In such a case the elbow ought of course to be dislodged from its resting-place, if possible, before anything else is done.

An interesting case of Spontaneous Evolution is related by Mr. Heslop, of Haverfordwest. The head and arm were descending together, and had actually become impacted in the pelvis, when a sudden and violent contraction of the uterus completely turned the child over and brought down the breech and legs; the labour was completed immediately. There could be no deception here: one arm was at first protruding externally, and the head presenting above it; suddenly the legs and breech presented themselves, to the astonishment of Mr. Heslop. This shows how wonderfully the powers of the womb will sometimes rectify matters, although these powers are not often to be trusted on such occasions. It shows also that some practitioners, who have lately attempted to substitute external manipulation for the internal process of turning the child in utero, may have been materially assisted in their efforts by the peculiar direction which they have given to the contractions of the womb. Instead of introducing

the hand and arm, and bringing down one or both legs, they have pushed up the presenting arm and shoulder with one hand, whilst with the other, applied to the abdomen externally, they have attempted as it were to turn the child over; or so manipulated as to give a fresh direction to the efforts of the womb. But although the practice may have been occasionally successful, we should never venture to trust to it if we could by any means introduce the hand and turn. It must be confessed, however, that when the shoulder and arm of the child are impacted low down in the pelvis—*jammed up* as it were in the pelvic bones—it is a difficult matter to get down the leg. We have occasionally met with some most awkward cases in our own practice, but have always hitherto succeeded by following the practice of our eminent and venerable colleague, Mr. Smith, of Leeds, viz., pushing up the end of a silk handkerchief, or a strong bit of linen, with one or two fingers or part of the hand, so as to get it round the back of the knee-joint. By perseverance you may *loop* this round the flexed joint, and work it downwards, so as to form a powerful loop to pull at; or you may get hold of a foot and *noose* it securely. Now you can *pull* with the handkerchief, whilst with the fingers of the other hand you can *push* the child upwards. The womb will sometimes seem to assist in the operation, instead of resisting the process.

It is rather curious to notice the difference of opinion, amongst even our leading practitioners, on the method and uses of Supporting the Perineum during labour. Some say, do not support it at all but let nature do the whole work; others say, support it well with the palm of the hand, and press the head forwards. But all these celebrated writers and eminent practitioners omit all allusion to the shoulder of the child, which is a frequent cause of rupture of the perineum for the following reason: The practitioner is bending the whole of his attention to the passage of the head; he supports the perineum very carefully, and gently presses the head forwards towards the pubic arch, so as to relieve the great strain on the perineum; but as soon as the head passes he gives up all attention to the perineum, and forgets entirely that it has to be stretched again by the shoulder. Moreover, if he did remember this circumstance, he cannot follow the edge of the stretched perineum with the palm of his hand from the cleft of the neck, as it gradually becomes more and more stretched in passing over the point of the shoulder. The consequence is, that although he has prevented the perineum from being ruptured during the passage of the head, he allows it to be ruptured by the passage of the shoulder. To support the perineum during the passage of the head and shoulders, and to assist the head to take that curve towards the pubic arch which nature points out, is so useful, that we shall not attempt to answer the arguments against the practice, but simply give our own views. When the head has advanced sufficiently low the occiput is placed in the sub-pubic arch, and there becomes nearly stationery till the rest of the head has

cleared the perineum. It is evident that as the occiput remains in one place, and the rest of the head advances, it must take a curve something like putting the long finger of a clock *backwards* from twenty minutes *to* 1. to twenty minutes *past* 12 o'clock; twenty minutes *to* the hour represents the edge of a perineum at the full stretch, and twenty minutes past the hour represents the point to which the forehead of the child is advancing. To support the perineum properly, therefore, you must not push it upwards so as to retard the passage of the head, but your object is to steadily push the head forwards, which you are obliged to do sometimes, by having the hand *much further back* than the edge of the perineum. Your object in short, is to make the head take that *curve* which it ought to do; or as it were, to carry the long finger of the clock backwards from twenty minutes to 1 to twenty minutes past 12. Surely when we see the obvious wish of Nature, we ought to assist her to accomplish it when we can do so, and not leave her to her unaided powers, when perhaps she is nearly exhausted. But we think that to support the perineum the palm of the hand is not so good as the back of the knuckles of the left hand, well wrapt up in a soft napkin. In the usual obstetric position, if you support the perineum with the palm the fingers are upwards towards the sacrum, and you can only *support* the part: if you wish to make the head take its natural curve you will have to *pull* it forwards with the palm of the hand. You cannot well *push* it forwards, but in *pulling* the head forwards you soon lose the edge of the perineum as it slips over the head, and you quite lose all feel of it as it gets into the cleft of the child's neck and again becomes stretched over the shoulder. Now here is the fault which many medical men commit. But if you fold the hand in a napkin and apply the *flattened knuckles* across the perineum, you can feel accurately with the disengaged thumb where the perineum is, and what degree of tension is now existing. This will regulate your own efforts. You can in this way support the perineum accurately up to the very edge, make the head obey its natural curve, and when the head has passed you can follow the edge of the perineum with the thumb, feel it again becoming stretched by the shoulder, and again be ready with the flattened knuckles to give it support as it stretches over the shoulder; in no other way can you follow it accurately.

It may be said that in first labours, the perineum is generally more or less ruptured. When we consider how essential it is for a woman to have a good perineum in order to support the parts above, it becomes the duty of the medical attendant to see that these ruptured parts are approximated and healed immediately after labour, now that the wire needle is such a safe and easy remedy. It may be years afterwards that the woman will suffer from this injury, the parts becoming more distended or stretched. If practitioners will but heal these ruptured parts at once, we shall not hear of such numerous cases of prolapsions of the womb in future. The operation

is so simple, that it will almost be inexcusable in the practitioner avoiding the trouble; a couple of needle sutures, a little smooth wood, and a few pierced duck-shot, wil lbe sufficient apparatus. The edges of the wound are easily approximated and kept in position; they unite most readily, and the parts are as strong as before.

In the use of the Forceps, it is curious to notice in some cases how easily the face may be made to turn into the hollow of the sacrum by twisting it to the right side of the pelvis, and in other cases, how this is reversed. When the face is a good deal to the left side, you will find that when the forceps are properly applied over the ears, it is actually sometimes easier to make the face take a circuitous turn round to the right of the mother into the hollow of the pelvis, than to the left of the mother over the rectum; consequently, it is a good rule, wherever the face is placed, to try to make it turn to the right of the mother, unless, indeed, it is already so much to the left that it seems an easy matter to carry it over the rectum in that direction into the hollow of the sacrum. It would seem as if the body of the child often descended into the pelvic cavity with the face a little twisted over one shoulder, that is, with the left shoulder to the right acetabulum, and its head twisted a little over this shoulder, because, in some cases, when even the face is decidedly looking to the left you cannot turn it over the rectum, but can easily turn it in a circuitous way to the right of the mother. Some writers, indeed, suppose that this is the turn that the child takes spontaneously in natural labour, when the face has entered the pelvis in a direction towards the rectum. This practice is well illustrated in a case published by Dr. Hamilton, of Falkirk. The face presented to the left side and the labour was tedious, making it advisable to use the forceps; the face could easily be made to turn as far as the rectum, but no further; it was, however, easily turned towards the right synchondrosis, thus making a sweep round the pelvis, and causing the labour to end easily. As a rule, therefore, it may be well in turning the face into the hollow of the sacrum, to make it do so by turning it round to the right of the mother, so as to avoid passing over the rectum; this, however, cannot always be done.

It happens in three or four forceps cases out of every hundred, that this instrument is too short. The Head has not only been elongated by long-continued pressure in its passage downwards, but its shape has been altered, so that the ordinary short forceps is not only too short, but will not fit the head. Dr. Graily Hewitt draws our attention to an instance of this kind, and has had made a forceps adapted to such cases. The blade is eight inches long (instead of $6\frac{1}{2}$ or 7), the curve that of a circle of 14 inches (instead of 10 or 11 inches); in other respects it is like the short forceps. We are glad to see Dr. Barnes recommending the forceps of Mr. Roberton, of Manchester, and believe it to be one of the best kinds of forceps used in this country. It is well known what a degree of pressure on the perineum is liable

to be exerted by most forceps, unless the practitioner take the greatest care and avoid pressing the instrument backwards. Of Mr. Roberton's forceps, Dr. Barnes speaks in the following terms:—"For some years he had used exclusively the forceps of Mr. Roberton, of Manchester. This instrument was admirably adapted to all the ordinary emergencies, whether the head lay at the brim, in the cavity, or near the outlet. The pelvic curve, the length of the blades, and the length of the shank, kept the perineum free from any strain whatever, beyond what was incidental to the passage of the head itself. The instrument added nothing to the danger of the soft parts." Now that the application of the forceps seems to be a more frequent practice than it used to be, it is necessary that we should make more generally known such opinions and such instruments as these.

Medical practitioners in general are not yet sufficiently alive to the danger of going from surgical operations, sloughing sores, and post-mortem examinations, to attend women in labour. If they will read Mr. Nunneley's treatise 'On Erysipelas,' they will find this subject handled with great ability. This author points out how intimately connected Puerperal Fever is with erysipelas. We think he might have safely extended his remarks to all bad surgical cases where the fingers of the practitioner may be the medium of contagion to the woman; indeed, in some of the cases detailed by Mr. Nunneley, we can trace the puerperal fever conveyed to the lying-in woman, rather from the sloughing sore and gangrenous wound than from simple erysipelas. For example, take the following quotation from Mr. Nunneley's book:

"I shall first mention, as instances of the probable inducement of puerperal fever from erysipelas, some facts related by Dr. Paley, of Ripon, but formerly of Halifax, and which came under his own immediate observation. A man who resided near Halifax some years since was affected with a most severe attack of erysipelas, which rapidly terminated in gangrene. Whilst the surgeon was in the act of dressing the sloughing sore, he was called off to a female in labour, to whom he immediately went; this patient, and five others in succession, died of well marked puerperal fever, although the disease was not then known in the neighbourhood, had not been for many years, nor had any other practitioners any cases. Very recently a man in the neighbourhood of Ripon had a similar affection of the scrotum, and immediately afterwards puerperal fever occurred in the practice of the medical man who had the care and dressing of this patient.

"Mr. Blagden has related the case of one of the midwives of the British Lying-in Hospital, who, a few days after attending in labour a woman who died of extensive peritoneal inflammation, was seized with a severe attack of erysipelas of the face. 'On the 16th of March, 1831, a medical practitioner, who resides in a populous parish in the outskirts of London, examined the body of a woman who had died a few days after delivery, from inflammation of the peritoneal coat of the uterus. On the morning of the 17th of

March he was called to attend a private patient in labour, who was safely delivered on the same day. On the 19th she was attacked with the worst symptoms of uterine phlebitis, severe rigors, great disturbance of the cerebral functions, rapid feeble pulse, with acute pain in the hypogastrium, and peculiar sallow colour of the whole surface of the body. She died on the fourth day after the attack, the 22nd of March, and between this period and the 6th of April this practitioner attended two other patients, both of whom were attacked with the same disease in a malignant form, and fell victims to it.'" (Nunneley on Erysipelas, p. 81.)

Now, in the cases related by Dr. Paley, the surgeon was called to the woman in labour *while in the act of dressing the sloughing sore*, and this woman and five others in succession fell victims to his practice. Mr. Nunneley goes on to relate numerous other cases much to the same effect, and we draw attention to this subject, owing to the carelessness with which surgeons often regard it.

It is too much forgotten or unknown how often the fingers may be smeared with what may seem innocent fluid, but which is highly poisonous, and may in a very small quantity prove destructive to a woman when placed in contact with the denuded and sore surfaces of the vagina or cervix uteri. You would not suppose that the serum found in a peritoneal cavity, for example, would do much harm if a minute portion were carried on the practioner's finger, or under his nail, into the vagina of a woman; but this might be a fatal piece of ignorance in the medical attendant, and we again warn our medical brethren not to attend women in labour when they are in attendance on bad surgical cases, or examining post-mortem subjects; they will inevitably cause puerperal fever of a fatal character to exist in their practices from time to time. If the reader will turn to Mr. Spencer Wells' case of ovariotomy in the present volume, he will find a remark which fully bears out what we have just expressed. In a case of ovariotomy he had a small scratch on his finger, made during the operation, which was inoculated only with the serum of the peritoneal cavity, but he suffered severely for two days, had severe rigors, lasting several hours, with intense headache, relieved by vomiting and copious perspiration, which lasted about eighteen hours. He further says,—"all experience has proved inoculation of the fluids of those dying of puerperal peritonitis, erysipelas, &c., to be far more dangerous when the bodies have been fresh than when putrefaction has commenced." Now we ask any candid surgeon, after reading this opinion, whether he thinks it proper and just to a woman, to be operating in a case of hernia, or any other such capital operation, and go to attend her in labour on the same day—nay, perhaps, to hurry from the operating room or the post-mortem examination direct to the labour. Is he not wilfully risking the life of his patient, after he has been informed by the best authors that such a practice is dangerous in the extreme. Dr. Simpson, of Edinburgh, in one of his lectures, uses the following

words:—"A gentleman who was formerly surgeon to a very large hospital, and also in extensive practice in midwifery, informs me that during the period of his surgical superintendence of the hospital, and when consequently often touching the discharges from all kinds of wounds and breaches of surface, puerperal fever was from time to time common in his private practice, and at the same time he saw many of his hospital surgical patients die with similar symptoms. Since giving up the surgical charge of the hospital, the occurrence of puerperal fever has ceased in his private midwifery practice." (Edinburgh Medical Monthly Journal, Nov., 1850, p. 429.) Such facts require no comment, and we leave it to operating surgeons to do as their consciences direct them.

If we look over the older treatises on Puerperal Convulsions, we are struck with the improvement in the modern treatment of these formidable attacks. Formerly, the patient was bled to a fearful extent, sometimes to the amount of sixty ounces. The same effect on the system is now rapidly accomplished by the simple exhibition of chloroform. In a case related by Dr. F. W. Wilson, it is interesting to see the remarkable effects of this remedy: the case was apparently desperate, the fits were occurring every quarter of an hour, labour had commenced, and the os was dilatable; chloroform was administered, and in a minute or two its effects were visible, and in ten minutes the fits ceased. The next thing to be done was to deliver the woman by the forceps; this was done well; and, except for another convulsion three hours afterwards, which was again relieved by the chloroform, the case ended satisfactorily. We would suggest that in such a case as this, in which labour was commencing and the os dilatable, not only should the chloroform be administered, but the woman should be delivered by turning the child the moment the hand can be easily introduced through the os uteri. It is of no use waiting till the child have descended low enough to use the forceps,—turning is actually in these cases a more easy and a safer operation than the use of the forceps. We would also suggest that in all suspicious cases, the urine should be examined for albumen, and if this be present, the practitioner must be prepared for such an attack. Albumen will often be present in urine made *before* but not *after* an attack of puerperal convulsions.

Sometimes a dreadful case of Puerperal Convulsions will occur when the os uteri is rigid and not dilated; you will then be tempted to bleed largely, as was Dr. Robert Harper, of Holbeach; but we would suggest that the chloroform be tried first, as it will often not only check the convulsions, but soften and relax the os uteri so as to enable us to turn the child. We may, in fact, use the chloroform whenever the convulsions come on, whether before, during, or after, the labour, with great success.

We cannot have a much better instance of the efficacy of chloroform in Puerperal Convulsions than the one related by Dr. Page, of Landport. Before labour, soon after labour, and sixteen hours after labour,

did these alarming convulsions occur, and in every instance did the chloroform succeed. It has been doubted whether chloroform be a safe remedy in the apoplectic kind of puerperal convulsions, where coma with stertorous breathing are present, and some even recommend that *bleeding* should precede its use. From this opinion we differ, and consider that we may use the chloroform in such cases with even more safety and success than in cases when, from any cause, the patient has been exhausted. Chloroform, in fact, is always safer when there is plenty of power in the system, than when there is little; it is a powerful sedative, and we think it much safer and more efficacious to be used without, and instead of, bleeding, than with it. Dr. Page's case is an excellent one, but perhaps he would have done even better without the bleeding.

In a case of Morbid Poisoning after Parturition, how does the poison get into the general circulation—is it absorbed from without, taken from without by the veins of the womb, or is it generated within the vein itself? It is known that pus may be injected into a vein without causing those violent symptoms which we might have anticipated; but when a vein is poisoned by some morbid poison applied to it, and immediately forms a fibrinous plug, and when this plug of unhealthy fibrin decomposes and generates pus, then the smallest quantity,—a drop or two,—will give rise to the most dangerous symptoms of puerperal fever, and at other times, to deposits of pus in various parts of the system. The pus, then, is supposed to be secreted by the poisoned vein, at the part originally affected. Mr. Henry Lee asserts, we believe with truth, that few blood-poisons are more deleterious than decomposed fibrin, and Dr. Richardson does not believe in the absorption of pus from without, but from within the vein, and that in such like cases there is a great increase of fibrin in the blood.

The world at large talks of the improvements of the age, yet it knows but little of the progress of medicine, some of the greatest improvements in which are the simplest, such as the substitution of Metallic Sutures for silk or hempen ones. It is quite singular to read some of the cases of wounds in which a bullet had lodged in some deep parts for years without any particular ill effects; but in the 'Medical Times and Gazette,' for last August, there is related a case where a small bullet had been lodged in the heart itself for some time! It is common enough to hear of bullets being lodged in other parts, but we never before heard of one having been lodged in the heart. This only shows how little irritating a metallic body is to living structures compared with silk or hemp, which would soon have caused purulent inflammation. It is equally singular to notice the length of time which some good idea takes to be brought into practical operation. Mr. Gossett cured a vesico-vaginal fistula in London, by the use of a gold-wire suture in 1834, and Mr. Wells heard Mr. Morgan, at Guy's Hospital lecture on the uses of platinum wire as a suture in the year 1840, and yet it has taken fifteen or

twenty years to bring these ideas forward. We may chiefly thank Dr. Simpson for the introduction of metallic sutures.

We must draw the reader's attention to the paper of Mr. Collis, of the Meath Hospital, on the subject of Vesico-Vaginal Fistula. If other surgeons can accomplish so easily as he does what he recommends in this operation, it will be considerably simplified. Why should not the same principle be carried out in numerous other operations of a similar kind, such as cleft palate, hare-lip, and ruptured perineum? Instead of simply paring the edges of a vesico-vaginal fistula like some operators, or bevelling them off like others, he *splits* them—separates the mucous membrane of the two surfaces, and exposes the raw intermediate structure so as to be accurately approximated. The mucous edges will thus be separated and a little everted, as the wounded and split edges are brought together by the sutures. The size of the fistula need not be enlarged, as there is no paring away of the edges. Good thread will answer as ligature; Liston's needles fixed in long handles, with the eye near the point, are the best to carry the thread through. Other things are carefully described by Mr. Collis in his paper, to which we would refer the reader. By these means we think this delicate operation may be simplified; but of course a great deal will depend on the nature of the edges of the fistula. Mr. Collis, we suspect, will sometimes find the edges too thin to be thus *split*, or too thick and hard, so that his operation will be adapted only to those cases in which the parts can be split and divided as he recommends; whereas, the paring or bevelling processes may be applicable to all cases. If the edges are very thick and callous, we suspect that it would be no easy matter to *split* and *evert* them, so as to bring the raw surfaces together. Mr. Collis, however, thinks that his operation is adapted to almost every case, and to many which could not be subjected to the older operations with any chance of success.

We have sometimes thought that some of the dreadful cases of Prolapsion of the Womb might be materially relieved, not simply by the now common operation of paring the edges of the perineum, and uniting them, thus reducing the size of the external orifice, but by also reducing the size of the relaxed and dilated vagina itself, which was one of the original causes of the evil. To pare off some portion of the vagina either circularly or longitudinally, and to heal this, would produce a cicatrix: a cicatrix produced either in this artificial way or by an accidental rupture would be very objectionable in a woman who was likely to have more children, as it is well known that it would be torn during the next labour, and would very likely lacerate some of the contiguous structures, which the cicatrix had most probably got hold of; but in women past the period of child-bearing this objection would not exist, and we may safely diminish the diameter of the vagina by snipping off one or more portions of the internal membrane, and uniting the separated edges by the wire ligature, the

same as in other parts. This has been done successfully by Dr. Roberts, of Manchester, in a woman aged 49, and therefore a case for such an operation. In a severe case of procidentia he "dissected off a triangular slip of the mucous membrane covering the posterior surface of the uterus, and brought the edges of the wound together by silver wire; removed a similar piece from the anterior surface and drew the edges together in the same manner." He then pared the edges of the perineum in the usual way and united them by means of hare-lip pins, in the manner recommended by Mr. Spencer Wells. Of course the closure of the external orifice would prevent the state of the vagina being accurately examined. The cicatrices of the mucous membrane on the back and front of the womb could not be properly examined, and we regret that this one operation was not done at two different times. We would recommend in future that the wounds of the mucous membrane be cured first, in order to see the amount of good from them alone, and afterwards that the perineum be proceeded with in the usual way.

Some years ago it will be remembered how fashionable it was to apply Caustics to the os uteri, to disperse enlargements, and to heal what were called ulcers. This practice has now fallen almost into total disuse; we do not, at any rate, hear much about it, but some of the ill consequences are now and then heard of. Dr. Whitehead, of Manchester, says, that in many cases the reduction of bulk in the os and cervix was followed by total closure of the orifice of the womb; so complete was this closure, that he had to open it with a lancet and and keep it open artificially till the healing was completed.

Dysmenorrhœa, when owing to constriction of the cervix uteri, is often a more troublesome, and generally a more painful disease, than most of the diseases of menstruation; and although we now understand its pathology better than we did, we are still obliged in many cases to have recourse to various methods of dilating the os and cervix uteri. Dr. Coghlan uses a neat kind of bistoury, by means of which he saves much time in dilating this passage. It is only carrying out the usual practice, but every little hint may be useful, and we think this may be so too. The cervix is sometimes so narrow that there is no room for the menstrual secretion to pass; the suffering therefore, almost resembles a kind of miscarriage. The uterus is even excited to a kind of parturient action, and the life of the woman is made wretched; you can hardly introduce the wire or stilet of a catheter up the passage. It is curious to witness the improvement effected in some cases by skilfully dilating the cervical canal; first, try to pass a No. 1 flexible bougie, or a bit of smooth pine wood, or a fine bougie of gutta percha. Dr. Coghlan's instrument consists of a central blunt or probe point, less than a quarter of an inch long, and about as thick as a No. 1 bougie, and proceeding from this are two cutting sides; the whole instrument is very small, and by pushing the pointed probe along the os and cervix, it acts as a guide to the cutting

sides, and a slight incision is made safely, accurately, and *laterally*; a fine leaden bougie of the size required is then introduced. This leaden bougie will irritate less than almost any other, and it can be shaped at the sides, so as to correspond to the incisions made by the cutting instrument, and will thus prevent them from contracting. The parts thus remain more or less permanently dilated, and at the next menstrual period the secretion has room to get away, and the relief will often be found to be great; if not, the process must be repeated. There seems to us to be one disadvantage in Dr. Coghlan's instrument, which is that it has no sheath to protect it as it is passed along the vagina. Dr. Simpson's instrument, on the other hand, is introduced into the cervical canal completely protected by its sheath, like a hernia knife; the cutting edge can then be brought from its hiding-place, and *as it is withdrawn*, can *nick* or *cut* the exact part which is required. In all these cases, however, we think that Dr. Coghlan's small leaden cervical pessaries will cause less irritation than pessaries made of wood, sponge, or most other materials, on the same principle as the metallic suture causes less irritation than the common sutures. There is another disadvantage in this instrument of Dr. Coghlan's, namely, that it does not begin its incisions as it is *withdrawn*, but as it is pushed forwards. Now, it is often much better to follow Dr. Simpson's plan, which is well accomplished by his *metrotome*, viz., to begin the incision very slightly *at the os internum cervicis*, and as you *withdraw* the instrument to *enlarge the incision downwards*, so as almost to completely divide the lips of the os uteri. It strikes us that, if these two suggestions of Dr. Simpson and Dr. Coghlan were combined, we should gain something, viz., the cutting operation of Dr. Simpson, and the leaden pessary of Dr. Coghlan. One disadvantage of the leaden pessary would be, that it would probably *drop out* when the patient stood up. A gutta-percha pessary might be extemporised equally well, and altered on the spot so as to suit the parts, but would not have the advantage of being metallic. What we have been writing only applies, of course, to those cases which are owing to mechanical obstruction at the os and cervix; but we think that there is more connection than is generally supposed between membranous and mechanical dysmenorrhœa, and therefore this same treatment is not altogether inapplicable to membranous dysmenorrhœa.

It sometimes happens that a young unmarried woman parts with what appears to be a Deciduous Membrane, the result of conception. It is necessary to be very cautious in giving an opinion in such a case, as it is now known, or strongly suspected, that the deciduous membrane of conception is not a new membrane, but only the mucous membrane of the womb thickened, and that it may exist occasionally in a fair degree of development in cases of dysmenorrhœa. We can easily imagine that in some cases the womb of the unmarried female, especially during the menstrual period, may have more blood than usual sent to it, or that the blood which is sent to it, although in

normal quantity, is used up in the production of hypertrophy of the mucous membrane. In such cases we shall have occasionally many of the symptoms of miscarriage, with sometimes a deciduous membrane expelled. Dr. Beatty related a case of this description to the Dublin Pathological Society; his patient parted with one of these membranes every month, for twelve months in succession. It strongly resembled a "decidua vera" and was evidently owing to a process similar to that which produced the true deciduous membrane.

In some cases of Amenorrhœa, when the system has been replenished with a sufficient quantity of blood, but where, from disease as it were, the womb has lost the habit of menstruating, it may be useful to apply a local stimulant, or something to excite the organ. But this must be done with great caution, if done by means of instruments. Dr. Simpson, in some of his cases, has *dry cupped* the interior of the womb. He introduces a tube, resembling in length and size a male catheter, with a large number of thickly-set small orifices, with an exhausting syringe adapted to its outer end. When the instrument has been introduced, the syringe exhausts the air and draws the mucous membrane of the womb up to, and even perhaps a little into, the small holes of the tube, and thus draws a little blood from the congested membrane. This may have to be done several days successively, and eventually it may be the means of restoring the organ to its usual habits. But in other cases it fails, probably because the ovaries require to be stimulated as well as the womb. This may be sometimes accomplished by causing the patient to wear an intra-uterine pessary, made half of copper and half of zinc, and measuring $2\frac{1}{2}$ inches in length. This pessary is fixed on a bulb which is kept outside the os uteri, and has a small hole at its under surface, into which a bougie or catheter can be fixed, so as to facilitate its introduction, all of which is described at page 357. We must acknowledge, however, that we have great doubts as to the permanent good effects of this kind of interference. We may stimulate any organ, and especially any glandular organ, to do extra work, but may we not be interfering too much in the good and gentle conservative powers which nature is generally willing to make use of, if we will but give her time and opportunity? The power of electricity in such cases has been much extolled, and by some disputed; at any rate it is a harmless measure, if not beneficial, and therefore we should prefer it to more violent proceedings. Dr. Althaus extols its powers, both in the form of Faradisation and the continuous current, but not in the frictional form. Faradisation seems to be the best method, and it should be persevered in for some time. Faradisation of the skin of the soles of the feet by means of wire brushes, or of the abdominal surface by means of moistened conductors, or the application of one moistened conductor to the nape of the neck and another over the pubes, seem to be the best modes of using this remedy.

In Ovarian Dropsy it is best not to tap except there is only one

cyst, and it is better never to inject with iodine if there are more cysts than one. The injection of iodine has certainly cured some of these cases, but we must remember that cases which have been injected with iodine are believed not to do so well if the cyst has afterwards to be removed. Therefore, we would recommend Dr. Clay's plan, not to inject with iodine if we intend afterwards to recommend the removal of the cyst by operation. But, on the other hand, if you intend to try the iodine, do not be satisfied with one or even two failures, as a third injection will sometimes succeed after two others have failed. Also remember never to inject too much iodine, as was often done in the early use of this treatment, but be satisfied with about two ounces of the Edinburgh tincture. It is somewhat curious, as Dr. Clay remarks, that this disease seems to attack young unmarried females oftener than older and married subjects, and is worse to treat in these younger subjects, being more solid and more rapid in growth.

In the operation of Ovariotomy it cannot be too much impressed upon us that great danger arises not only from detaching the ovarian cyst from contiguous parts when adherent, but also when the contents of the cyst escape into the peritoneal cavity; the fluid is sometimes peculiarly irritating, and is supposed to have even some malignant property. Its effects, when spread over a large surface of peritoneum must be highly irritating in some cases. Is it right, therefore, in a case of pregnancy accompanied with ovarian dropsy, to attempt the removal of the cyst during gestation? We think this question may easily be answered. It must be a very peculiar and remarkable case which calls for the operation during pregnancy, and even tapping ought never to be performed in such a case, except under some peculiar and extreme circumstances. We should consider the danger of meddling with ovarian cysts during pregnancy so great, except perhaps in some cases tapping them, that we should even prefer bringing on premature labour as the less evil of the two.

It is now well known that Metallic Ligatures cause much less irritation than the common ligatures; bullets even have been known to be imbedded in parts for years without causing great inconvenience as before mentioned. These metallic wires used as ligatures cause adhesive rather than suppurative inflammation. We should be glad, therefore, to see them used in tying the peduncles of ovarian tumours and ovarian cysts instead of the usual ligatures. It is possible that the time may come when such a peduncle may be tied with a wire ligature which may safely be left in the abdomen without exciting mischief. Mr. Redfern Davies used a silver wire to compress the veins in a case of varicocele, and when he tried to remove it he failed, and therefore cut off the wire as short as he could. In a week the part was healed with the wire inside the wound, the varicocele being perfectly cured. (See page 176.) In many ovarian cases the ligature causes great mischief, and we would suggest that more attempts should be made to use the metallic ligatures.

The treatment of Asphyxia in Infants just born is an important question, and one on which the opinions of medical men are much divided. Some recommend the hot bath, and some a cool surrounding air. Many years ago Dr. Edwards, of Paris, very decidedly spoke against the hot bath, and indeed, we believe against all increase of temperature applied to the surface, asserting that this increase of artificial temperature in the child only caused a kind of waste of oxygen, which was required for other purposes. He had found that new-born mammiferous animals die most slowly in water of a temperature of about 60° F., which is nearly cold, and most rapidly in water of blood heat. Dr. Snow explained this fact as follows:—The heat stimulates the capillary circulation, and thus promotes the deoxygenation of the blood in a more rapid manner than when the surface is cool. Thus the oxygen is, as it were, wasted rapidly and uselessly. Moreover, we think that, in some slight degree, the skin must be a respiratory organ. Look, for example, at the reddened oxidized blood in the cheeks of a person who is walking in a cool and brisk wind. The hot bath, therefore, to say the least, is a very doubtful remedy in cases of asphyxia in new-born infants, and for our own part we never use it. There was an interesting discussion on this subject lately at one of the meetings of the Medico-Chirurgical Society, after a paper by Dr. Waters. This gentleman proved that the lungs of animals which had been put into hot baths were more congested, firmer, and heavier than in animals not so treated. Both sides of the heart were loaded with blood in those which had been submerged in hot water, and in the lungs the blood was sometimes coagulated, and was, indeed, less fluid generally, and more disposed to coagulate when removed from the vessels. In no instance did the hot bath produce a respiratory act. All these phenomena are directly adverse to the renewal of respiration in asphyxiated infants. *Heat*, in fact, does not excite respiration so much as *cold*. Nature herself excites the first respiratory act by bringing the child from a hot to a cold medium, and we all know how cold, suddenly applied either by a dash of cold water or air, will stimulate the respiratory act. It is curious sometimes to watch the effect of a single respiration in the infant—how this almost necessitates others, especially when the air of the first inspiration is gently expelled by slow compression of the chest; when the air is thus gently expelled, it is almost certain to re-enter by the expanding chest causing more or less vacuum. The art of treating asphyxia in the infant is thus simplified. Let it be exposed to a cool air; employ slight friction of the skin; tickle its nose or fauces with a feather; give a gentle slap or two on its buttocks or elsewhere; dash a little cold water on its face or body; and, if these means fail, introduce a small catheter into the trachea, or apply a little clean linen over the mouth, and *very gently* distend the chest with air. Don't do this violently, but extremely gently. You want simply to dilate the respiratory organs, in order to be enabled to squeeze the air out again,

and thus to create such a partial vacuum that the air must necessarily rush in again. The fault generally committed is, that this is done *too much*—the practitioner often blows down the trachea of the infant as if he were blowing up a bladder, and he does this *too rapidly* and too continuously. The process differs a little when it has to be used in the adult after drowning. In this case the lungs have to be emptied of bad air *first*, but in the infant no air at all has yet entered. If it were a more ready method, it would no doubt be useful in some of these cases, to use oxygen itself. In our 11th volume, Dr. George Wilson, of Edinburgh, says that oxygen can in a few minutes be generated for this purpose. By mixing chlorate of potash with a tenth of its weight of black oxide of manganese, the black oxide of copper, or certain other oxides, and applying heat to the retort by a spirit-lamp, you can obtain 200 cubic inches of oxygen in four minutes. The gas begins to come off in a few seconds after the application of the flame, and literally gushes in a full stream till the whole is evolved. Why is not this simple and inexpensive method more frequently utilised by practitioners?

It is surprising to notice the rapidity with which Hydrocephalus in children sometimes sets in; but the reason is, that the observer has not watched its stealthy preliminary stages. The cerebrum and intellectual faculties in a child, those portions of the brain situated above the cerebral ganglia, are not so necessary to its existence as might be supposed. Provided that the base and lower portions be tolerably healthy, the organic functions will be carried on tolerably well. The upper portions may be considerably unhealthy without those severe symptoms which would at once attract the attention of the medical attendant. Hence, in a child of a tubercular disposition, we ought always to be on our guard against tubercular effusions on any part of the brain, and pay greater attention to head symptoms, which in another kind of case might not be required. As Dr. West says, "whenever the tubercular cachexia is much developed, a very small amount of cerebral disease is quite sufficient to destroy the patient, and the slight symptoms during life will be found to be the expression of a very small amount of local mischief." The correctness of this opinion, however, depends on the part of the brain where the tubercular disease exists. A considerable amount of disease may exist *high up* in the brain, without causing death, but let a small amount of disease attack the *lower parts* where the par vagum on each side arises, and death will not be long in ending the scene. You may often see a child playing about without pain, but every now and then apparently oppressed with something like giddiness or confusion, and perhaps the next day apparently all right; this may go on for weeks. There is tubercular disease in an incipient degree in the meshes of the pia mater on the upper parts of the brain, but the base is pretty free. The diseased parts are not those which give rise to pain, none of the cerebral ganglia are yet much involved, and neither

sensation, nor motion, nor even intellect, are yet much affected; but examine the *pulse* repeatedly, and you will find it occasionally irregular, sometimes too slow, sometimes too rapid; the *iris* is also rapidly variable, or too dilated; the stomach is irritable, drowsiness is more frequent, the child in the midst of his play goes and rests his forehead on the sofa or chair. The tubercular affection is rapidly spreading to the base of the brain, and you must be accurate in your diagnosis. The best available treatment is to endeavour, in the early stages, to prevent the progress of the mischief. You must treat the case as a scrofulous one, and not an inflammatory one. Few of these cases will bear the old methods of depletion.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON FEVER IN THE ZAMBESI.

By Dr. Livingstone.

[At the Epidemiological Society, Dr. McWilliam read the following interesting note of Dr. Livingstone on the fever of this district.]

In the typical cases given in Dr. McWilliam's "Medical History of the Niger Expedition," the gall bladder was found distended with black bile, and if my memory does not deceive me, most of the cases treated with quinine at an early period of the disease, either recovered or were subjected to the milder or intermittent form of fever. In 1850 I adopted the plan of giving quinine, mixed with a purgative, as the first step of the treatment, and was successful in the cases of two of my own children, and an English party whom we found at Lake Ngami, and of whom one had died before our arrival. I have lost the notes of my reason for adopting the practice, but I have been successful in every case I have met with since. The prescription employed is resin of jalap, calomel, of each eight grains, quinine c. rhubarb, of each four grains, mix well together, and when required make into pills, with spirit of cardamom. Dose from ten to twenty grains. The violent headache, pains in the back, &c., all are relieved in from four to six hours, and with the operation of the medicine there is an enormous discharge of black bile; the patient frequently calls it blood. If the operation is delayed, a dessert-spoonful of salts promotes the action. Quinine is then given till the ears ring, &c. We have tried to substitute other purgatives instead of resin of jalap, and calomel; but experiments have only produced the conviction that aught else is mere trifling. No strength is lost in our march up the river, of 600 miles on foot. An European would be stricken down in one day, and the next after the operation of the remedy would resume his march on foot. In some very severe attacks it was necessary for the patient to travel on a donkey, but after two or three days he would prefer to tramp it. We tried Warburg's tincture, which has a great reputation in India, but it causes profuse sweating and does not cure the disease: the strength is also impaired. We had a good supply, by the kindness of one of our nobility, but I am compelled to say that it did not answer our expectations. The daily use of quinine is no

preventive. We have seen many cases occur when the person was on the verge of cinchonism. I employed the foregoing remedy with success on the west coast, but made no fuss about it more than make a general statement in the "Missionary Travels." I was not quite sure that our fever was identical with that Dr. McWilliam encountered in the Niger, but the melancholy fate of a party of six missionaries at Linyanti, where six out of nine Europeans and four native attendants perished in the short space of three months, makes me fear that it is the same complaint as that which destroyed the officers of Commodore Owen in the Zambesi, those of Captain Tuckey in the Congo, and the crews of the great Niger Expedition in that river. My companions, Dr. Kirk and Mr. C. Livingstone, entertain the same idea of the value of our pills as I do. We wrote a paper for one of the Medical Journals, but the above sad case makes us anxious that the remedy should become more extensively known than it has been, and I do not know a better plan for effecting this than by communicating it to Dr. McWilliam. Those who may try the remedy will do well to remember that the above doses are for strong adults. I cured myself and native companions in this way during my long journeys between 1852 and 1856, and that the remedy has no bad effects on the system, may be inferred from the fact that I have had no regular attack of fever since my return. I have had little illnesses, probably from exposure to malaria in its most intense forms, but nothing like what I formerly experienced; and I am of opinion that what we were all taught, not to give quinine till we had used the preliminary measure of relieving the bowels, was a mistake. [Query: Might the remedy not be applied to some of the fevers at home that arise in unhealthy localities?] Around every village in this country there is a large collection of human ordure during the dry season. This is swept into the rivers by the heavy rains, and you may guess the effect from hundreds of thousands of villagers. The natives here do not drink water as the natives do on the banks of the Thames, but make holes in the sand to draw from. Possibly this has as much to do with the origin of fever as it has at home.—*Med. Times and Gazette, June* 29, 1861, *p.* 684.

2.—ON THE DIFFERENCE BETWEEN *CURING* AND *TREATING* A FEVER.

By Dr. Peter Mere Latham.

Let us consider fever in its largest sense, and see first what it is to *cure* it, and then what it is to *treat* it. This difference will display the difference between *cure* and *treatment*, as applied to the management of all other diseases whatever.

Take cold, and heat, and perspiration, occurring interchangeably, whether regularly or irregularly, and in any manner or degree; and a frequent pulse, whether strong or weak, and pain in the head with

wakefulness and delirium, or drowsiness and topor; and the tongue dry and red and clean, or sordid and black; the breathing hurried, and perhaps the bronchi wheezing, the abdomen tympanitic and painful on pressure, and perhaps a sanious discharge running from the bowels; take these symptoms on any day early or late in the course of the disease; take them all in a heap, and regard them as one, and make them serve collectively for the single aim of the single remedy, and that remedy quinine. This is pre-eminently the cure of fever; and its success, which comes suddenly when it comes at all, must be a sort of triumph.

Again, take all these same phenomena which tell of disorder and disease in the vascular system and the nervous system, and in any or every organ of the body; take them, not as they appear on any one day, but as they ebb and flow and fluctuate every day and night for more than twenty days and nights in succession; and watch each, and try to interpret the meaning of each, and judge which is most perilous in itself, and which bears most perilously upon the whole disease; and which is most within reach of a remedy, and which is most urgent for relief; then choose and apply, not *the* one remedy, but remedies many and various, or few and simple, according to the aims proposed; and take care the right remedy of to-day be not the wrong remedy of to-morrow; pure air to breathe, and pure water to drink; affusions or ablutions of the whole body, cold, warm, or tepid; lotions or fomentations of this part or that; remedies for pain according to its nature and seat; expedients for preventing sores, and expedients for healing them; aperients, and astringents, and mercurial alteratives, according to their several needs; also opium, that great saver or destroyer of life as it is rightly or wrongly given in fever, in its various uses. These are the sort of implements which the physician has to work with when he has to *treat* a fever, and being thus vigilant and analytical about the bedside emergencies of fever, and dextrous in ministering to them, he saves many lives.

And this is pre-eminently the *treatment* of fever; but it may be, and often is, one part of it only. For, after all has been done that need, or can, be done, in the way of thus noting particulars and ministering to them, lives cannot be saved without engaging observation and treatment in larger aims. More in one case and less in another, more in one stage of the same case, and less in another; but in some stage of almost every case, we have to take the sum of the symptoms, and calculate whether, upon the whole, they mean strength or weakness, vital reaction or vital yielding; and to make choice accordingly, whether it be right to restrain, to lower, to deplete, or to support, to raise, to stimulate; whether to give calomel and antimonials, or to use some kind of bleeding, or to give wine, or brandy, or ethers.

This is the *treatment* of fever in another of its parts. Both together, that which is analytical and that which is summary, comprise the *whole treatment*. If it succeed, its success is after the lapse

of many days, and the trial of many hopes and fears. And when success comes at last, it produces nothing like exultation and triumph, but rather quiet satisfaction and thankfulness.

Such is the difference between cure and treatment, when we come to follow them out in their operations, and to think what the one is and what the other, practically and in truth. The difference is so great, that if this vast class of diseases, called fevers, belonging to the whole world, and always existing and raging somewhere, could be taken at once out of the domain of *treatment* and transferred to the domain of *cure* now and for ever; and if, instead of needing remedies, as many and various as are the times and places, and men and circumstances wherein they arise, they were found capable of being safely and successfully consigned to one and the same remedy always and everywhere, and in all persons, the practice of medicine would suffer the nature of a revolution. The thoughts, studies, habits, and feelings of medical men would be utterly changed.

So great a revolution is not likely to happen. But a man need not have grown old in the practice of medicine to bear witness to its having undergone considerable changes; and the diseases which have been the objects of them are especially those we have been speaking of; and the changes themselves have still respected *treatment* and *cure*. They have not, however, amounted to anything like a substitution of one for the other; but have only involved measures of more or less, in which treatment and cure have been mixed together, and that not always and everywhere, but partially and from time to time, yet for long times together, and in this place or that; yet it may be over many countries. Thus, febrile diseases which have a contagious or malarious origin, or which arise from causes so occult that they seem spontaneously engendered; also, typhus and typhoid fevers, scarlatina, erysipelas, spontaneous febrile hemorrhage, and purpura, will for a time, even for a series of years, and over a great portion of the world, be capable of successful management by remedies addressed to the circumstances and conditions of individual cases—that is, simply by treatment. Again, some or all of these diseases in the same place, during another series of years, will yield less satisfactorily to the same remedial method. The indications will be as well chosen as before, and the medicines used will fulfil their immediate purpose, but the whole disease will not decline as easily or come to an end as soon or as completely as before. And then physicians will not be able to make up for the failure of their tried resources by seeking and finding new indications and new remedies rationally suited to them, but by using a special remedy which acts and cures nobody knows how or why—even the great specific cinchona. Then cure has become the needful auxiliary of treatment; and a special remedy given on the faith of a large experience has completed the imperfect work of remedies addressed to rational indications.

Now there are subjects upon which the most sober and practical minds cannot help speculating a little beyond what they know. Sure and great results—yet familiar and common, and procured at will and by certain means, but in an unaccountable manner—naturally set us thinking and forming notions how they come to pass; and then it is safest and best to fill up the gaps of our knowledge from analogy. When we are treating a disease, our aim may be to raise the pulse or to lower it, to cool the skin or to warm it, to purge the bowels or restrain them, to augment the secretion of this or that gland or to diminish it or alter its quality; and as our remedies do their appointed work, we witness the whole disease brought to an end. But, when we give cinchona for an ague, we have no single aims; we give it for the whole disease, and the cure follows. Nevertheless, it is according to analogy to believe that it hits a mark of its own, which is more particular than any within the scope of our discernment. Thus cinchona, and all so called specifics, may work after the same manner with other remedies, which are better understood. They may all equally produce their great results by intermediate operations; only in the one case the intermediate operation is seen, and unseen in the other; aimed at in the one, and unaimed at in the other.

From this view of the matter, it would turn out that our distinction between *cure* and *treatment* is not in the nature of things, but in the imperfection of our knowledge.

Further, from the same analogy, we may fairly believe that there is a certain morbid element, of which cinchona is the specific antidote; that it exists most pure and unmixed in ague, but that it may add itself as an accident to other elements whose property is to produce other forms of disease; and that wherever it exists, and in whatever combinations, its power is still predominant, and cinchona is still needed to bring the disease to an end.

These things may be as analogy would suggest. But in practice there is a mischief in conceiting ourselves wiser than we are. Granted that of the many morbid actions and sufferings which make up the disease, one only may be reached and swayed, or annulled, or curatively modified, by the one special remedy; granted, that thus the complex disease may cease by removal of some one main element, and by some intermediate operation of the remedy; but inasmuch as we know no such element of the disease, and see no such intermediate operation of the remedy, they must be to us as if they did not exist. We cannot construct aims and indications of practice out of hidden things, but must be content for the present to regard the whole disease as a single comprehensive indication, and so prescribe for it the single remedy and expect the cure. We shall perhaps know better some time; but no good will come from our pretending to know better now. Patience under imperfect knowledge is no proof of an unwise mind.—*British Med. Journal, Aug.* 31, 1861, *p.* 222.

3.—ON TYPHUS FEVER.

By Dr. Bell, Physician and Clinical Lecturer, Glasgow Royal Infirmary.

[Dr. Bell, in the series of papers of which the following extract is the conclusion, endeavours to prove that the typhus and typhoid forms of the disease are identical as to cause, that is, they both come from the same morbid poison. The doctrine which prevails as to the cause of fever (namely, that a morbid poison is absorbed into the system) is not of the slightest value in the treatment of the case. Dr. B. says:]

With regard to the theory of Liebig, that this morbid poison acts as a ferment, I beg to state that it is an hypothesis which is totally unsupported by a single apposite fact, but, on the contrary, is opposed to well-established principles. For example, if fermentation, or any process analogous to it, should be set up in the blood, such a process could never come to an end during life, in consequence of the ever-changing character of the blood; the position of this fluid being perfectly dissimilar to that of the fixed quantity of saccharine fluid in the brewer's vat. Even here let a stream of this fluid be constantly escaping from the vessel, and a new portion be as constantly added to it—why, fermentation would never cease so long as these changes were in operation. This is precisely the case regarding the blood in the bloodvessels, and it appears to me perfectly inexplicable how that, with this *fact* so prominently patent to all, the theory of the distinguished chemist should have been entertained for a single moment by physicians; and it is still more astonishing that the theory should have had the effect of changing the nomenclature of the whole class of diseases arising from morbid poisons. The term *Zymotic* is a standing reproach to the medical profession, because it shows how easily we have been captivated by a mere theory, and thereby so blinded as to overlook the well-established laws and facts connected with the process of fermentation. But, apart from considerations regarding the truth of this hypothesis, even supposing it to be correct, let me ask what advantage does it confer in the treatment of the disease? Does it suggest the shadow of an idea as to the management of a single case, or the removal of a single pathological condition which exists? Assuredly not. On the contrary, the adoption of this zymotic theory positively may do much harm; it tends to paralyze all investigation into the pathological states which actually occur. The notion that a process of fermentation must *be completed* leads the practitioner to let the disease run its course, without an effort to ward off or remove those lesions, both of function and texture, from which death results. We have seen that death in fever very seldom, if ever, arises from mere functional disturbance; but invariably, or very nearly so, from actual structural changes—viz., congestion or exudation into the brain, softening and degeneration of the muscular tissue of the heart, ulceration, and many other associated lesions of the small

intestines, or from serious disease of the pulmonary organs. Let me inquire how fermentation produces such morbid results in solid tissues? As practical physicians we must discard all theory in the treatment of fever as a mere "will o' the wisp" which will lead us astray. We must base our treatment on the knowledge which we obtain in our post-mortem examinations, combined with careful clinical observations made during life. The facts so obtained must be our sole guide, and we must never neglect their valuable instruction. They teach us the following important lessons:—1st. That typhus produces an effect upon the whole frame; that the muscular system, the brain and nervous system, the circulatory system, the digestive or nutritive system, and the respiratory system, are all more or less deranged. 2nd. Besides this, observation teaches us that persons attacked by typhus are not uniformly influenced. 3rd. We see that all the different functions or systems are not uniformly attacked with equal severity, but that, in some cases, and during some epidemics, one series of lesions are more frequent or more severe than in others; that whilst, as a general law, disease of the mucous membrane of the intestines is the usual pathological manifestation of the disease, yet diseases of other organs and other tissues frequently supervene, and prove destructive to life.

Let me next point out very shortly the practical value of these clinical facts in the treatment of typhus:—1st. The fact that typhus produces an effect on the whole system, leads us to direct attention to the condition of every organ in the body. The muscular system becomes impaired; we therefore must take care to prevent exertion; we should confine the patient to bed; cause him at an early period to use the bed-pan and the urinal. Though he may be perfectly able to get up to perform these necessary evacuations, yet as the time may come when he will be unfit to do so, it is a good plan to accustom him to the use of these utensils whilst he has sense to guide him; and besides this, when care is taken to preserve the patient's strength in the early stages of the disease, the subsequent prostration will be modified.

2nd. The knowledge that the digestive system is impaired points out the necessity of great attention being devoted to the character of the food and drink, so as to give the patient aliment best calculated to undergo easy digestion. Again, the almost invariable implication of the mucous membrane of the small intestines in the disease, demands that every care should be taken to avoid the exhibition of any article of food or medicine which is likely to produce irritation of the alimentary canal. The knowledge of the existence of this intestinal lesion also guides us to the early employment of means calculated either to modify the extent of its development or remove its effect.

3rd. The fact that the brain is more or less affected points out to us the necessity of taking steps to prevent all causes of excitement. The patient should not be annoyed with business matters; there

should be no conversation allowed in the sick-room calculated to excite his mind; the head should be shaved and kept cool by cold applications; the bed-chamber should be kept quiet, and somewhat darkened. Those symptoms to which I have referred as indications of severe cerebral disturbance should be carefully attended to—such as sleeplessness, subsultus, cerebral breathing; because it is only at the earliest stages of these complications that we can hope to secure success from treatment.

4th. The knowledge that the respiratory system is also affected in fever, leads us to adopt measures to maintain a constant supply of pure air in the patient's apartment, avoiding all currents, and cautions us to make a careful examination of the chest at every visit.

5th. The fact that the heart becomes diseased in fever instructs us to make it a principal point in our attendance to ascertain frequently the state of its impulse and sounds, so as to be able to avail ourselves of this only safe key to the administration of stimulants.

6th. The evacuations must also receive a careful attention. The peculiar loose stool is most significant; and the important duty of attending to the state of the bladder must never be overlooked. These are points which should never be left to the nurse. The practitioner must always institute a personal examination.

But the clinical fact that fever affects the whole system is not of greater practical value than the other fact, that these different systems are neither equally nor uniformly disordered. The knowledge of this important circumstance leads us to examine every case with the greatest care, so as to be able to detect at the very earliest moment any tendency to the undue disturbance of any particular function or organ.

These facts instruct us to watch carefully, not only the peculiar manifestations in each individual attacked with fever, but also the tendencies which are exhibited in different epidemics to affect some organs more than others.

Whilst we know that, as a general law, the fever poison impairs the whole system, and that it produces its local manifestation on the mucous membrane of the small intestines, we also know that it attacks other tissues and organs secondarily, and that these secondary lesions are by no means either constant or of uniform intensity, but that at the same time they are frequently much more severe than the primary intestinal ones, and often prove more fatal.

It must follow from these considerations, that fever cannot be correctly treated by any *routine practice;* on the contrary, it is perfectly clear that whilst every attention must be given to the usual anatomical lesion in every case, yet each individual patient must be treated according to the character of his constitution, and the nature and extent of the various secondary complications to which I have directed attention in this series of contributions—papers which I regret to say abound with many imperfections. The only apology

which I can plead is, that they have been written at considerable intervals, amidst the usual interruptions of professional life.

But whatever faults may be detected, I beg to remark that I have endeavoured to place before my readers the result of nearly twenty-five years' experience of a disease in which I have always taken a deep interest—a disease which I have had ample opportunities of studying under all circumstances, viz., in the rural district, in the manufacturing and mining village, in the overcrowded and densely populated city, in the private dwelling, and in the public hospital; in its sporadic forms, and in its epidemic outbreaks.—*Glasgow Med. Journal, April*, 1861, *p.* 57.

4.—*Fever in the Crimea.*—In a Treatise on Fever lately published, "it is shown by Dr. Lyons that considerably more than half of the whole deaths in that splendid army of over 300,000 men which France supplied in the late Russian war, was caused by fevers, or the diseases secondary to fevers; whilst on the side of England, 10,000 men of the flower of the British army were carried off by the same class of affections in seven months during the first winter campaign. From the 1st of September, 1854, to April, 1855, 4228 men perished from disease in the hospitals of Scutari alone, of whom but 359 died from wounds. Such, rather than balls and bayonets, are the true war plagues. The destroying angel of an army is fever, not gunpowder."—*Lancet, May* 21, 186 *p.* 515.

5.—PATHOGNOMONIC SIGN OF SCARLATINA.

For some years past, M. Bouchut has been in the habit of pointing out in his wards a curious sign which assists in the discrimination of scarlatina from measles, erythema, erysipelas, &c. It consists in a vascular phenomenon, proportionate in intensity to the extreme contractibility of the capillaries: we refer to the enduring *white stripe* produced at will by drawing the back of the nail over the part of the skin in which the eruption exists. Pressure with the nail or any other hard substance upon the exanthematous surface produces a white streak which lasts one or two minutes, and sometimes more. Figures may thus be traced upon the skin, the lines of which are conspicuous from their whiteness. With a blunt probe or pen-holder, the diagnosis of the disease may be distinctly inscribed on the integument, and after a minute or two the word *scarlatina* disappears, when the uniform tinge of the eruption again invades the written surface.

This phenomenon is observable in scarlatina only. The scarlet hue of measles is not uniform, the eruption consisting of mottled patches, with very slight elevations separated by interstices of healthy skin. In measles the procedure we have described would produce an alternately red and white streak, enduring a much shorter time than in

scarlatina. In erysipelas, in the redness induced by a mustard poultice, in solar erythema, the white line we allude to is not visible; and without attributing to this sign an undue degree of importance, it may be said to supply one more element in the determination of the characters of the eruption of scarlatina.

Among the young patients in whom this symptom was noted this year, we observed one in whom scarlatina occasioned a delirium similar to that of meningitis, and who recovered, without any other prescription than mixtures with two ounces of syrup of mulberry. In this case, pressure with the finger left a distinct and lasting white line, especially on the second day of the eruption. The instances which place the fact beyond question may now be numbered by hundreds, and the phenomenon invariably appears when the eruption is complete. It is, moreover, not a little singular that when the exanthema has faded, and the skin has resumed its natural rosy hue, white lines may still be traced by friction with the finger, and last longer than on those parts of the skin to which the eruption did not extend.

M. Bouchut has sought for the cause of this phenomenon. How is it that in a cutaneous eruption, consequent upon universal capillary congestion, sudden and lasting discoloration can be induced by friction? He considers this to be due to a considerable increase of the contractile power of the capillaries, proportionate to the intensity of the disease, the regularity of the eruption, and the amount of vital power. The capillaries contract, and expel their contents; hence the *white stripe of scarlatina.* This excessive tonic contractility of the bloodvessels is further remarkable, inasmuch as it is peculiar to this disease; once brought into play by pressure, the vascular contraction seems to last one or two minutes—a circumstance which does not occur in any other morbid condition.—*Journal of Practical Medicine and Surgery.—Medical Circular, April* 24, 1861, *p.* 302.

6.—ON THE ACTION OF SESQUICARBONATE OF AMMONIA IN SCARLET FEVER.

By Dr. John M'Nab, Bunessan, Oban.

[This remedy, no doubt, is very valuable in scarlet fever, often from the very commencement of the attack, not only given internally, but used as a gargle. In some severe cases Dr. M'Nab used it as follows:]

When given as a gargle, it was employed in the proportion of two drachms of the salt to six ounces of water—the throat to be gargled with the mixture every four hours. When ordered internally it was prescribed in five or ten-grain doses, according to the age and strength of the patient, and given three times a-day. This system of treatment, combined with the general therapeutical indications, was as a general rule attended with marked relief, particularly if the patient

was of age sufficient to become impressed with the necessity of using the gargle, and provided the disease had not already involved the throat to such an extent as to interfere with deglutition, and produce organic change and disorganization.

The specific therapeutical effects of sesquicarbonate of ammonia in scarlet fever seem to depend upon its direct influence on the special blood-poison, from the presence of which in the system the disease arises; for, as scarlet fever is essentially the effect of a specific contagion on the system, and as the sesquicarbonate of ammonia changes or modifies the action of that blood-poison, so there is every reason to believe that the remedy acts specifically, by neutralizing the so-called materies morbi. But, apart from any specific influence it exerts, it seems to be of essential service in the treatment of the disease, in consequence of its diaphoretic and sudorific properties, and from the circumstance that it is a prompt and diffusible stimulant, especially of the nervous system, while it possesses the advantage of not accelerating the circulation. Anasarca, which is the most common and the most formidable of the sequelæ of scarlet fever, is not so frequently experienced in those cases where the sesquicarbonate is administered in the early stages of the disease, as where it has not been given at all, or given only when the disease has reached its acmé. This favourable result is probably due to the diaphoretic action of the remedy, which, when freely administered, relieves the kidneys of the undue action they would have to perform in eliminating the blood-poison through the urine. "But it need be no surprise at all that there are certain remedies found which operate on the human body with a power not yet explained or understood, since there are likewise so many diseases whose nature and causes are entirely unknown." Let us hope, however, that in the onward progress of scientific medicine, the action of remedies, their properties, and the cause of their efficiency in the cure of disease, will be better understood; and that disease itself, with its deleterious influence on the human race, will be stripped of its virulence, mitigated in its course, and governed by laws fixed and determinate as the periodic tides of the ocean.—*Edinburgh Medical Journal, Oct.* 1861, *p.* 338.

7.—*On the use of Benzoate of Ammonia on Renal Dropsy succeeding Scarlatina.* By Dr. J. Taylor, Surrey-place, Old Kent-road.—It frequently happens that the sequelæ to scarlatina occasion more trouble to the physician than the original disorder, more especially in cases of anasarca with albuminous urine, the process of cure being often tedious, and not unfrequently unsatisfactory both to the patient and practitioner. If, therefore, any addition or improvement can be made by means of which such morbid conditions may become more amenable to the remedies employed, society will be benefited and the physician gratified. Having had many cases of the above

nature during the past and the present year, I have prescribed the following draught (for a child six years old) with more than ordinary success. The remedy being not unpleasant, is readily taken by children:—Benzoate of ammonia, five grains; spirit of nitrous ether, fifteen minims; syrup of tolu. one drachm; camphor mixture to one ounce; to be taken three times a-day. This, with a purge of the compound jalap powder, has answered admirably, the urine becoming free from albumen in a much shorter time than usual, and the patient's strength being afterwards recruited by the ammonio-citrate of iron.—*Lancet, Oct.* 12, 1861, *p.* 361.

8.—*On Arsenious Acid as a substitute for Quinine in Fever.* By J. Turner, Esq., Surgeon to H.M. Brigade, Bombay Horse Artillery. —Mr. Turner has employed arsenious acid for twenty years in the treatment of intermittent fevers, and on account of the great drain upon the cinchona tree, its failure in India, and his strong opinion as to the equal if not greater value of arsenious acid in the above-named diseases, he now brings the results of his experience before the profession. He considers the fears of an inconvenience or danger arising from the remedy as much exaggerated, and instances the case of a child of nine months, to whom he gave twenty minims of the arsenite of potash within ten hours, repeating the dose on the following day, with the only effect of curing an obstinate quotidian intermittent. Mr. Turner's success was so marked, that in 1860 the Director-General stated that Mr. Turner should be thanked for "drawing attention to his successful treatment of intermittent fevers by large doses of arsenic, and steps should be taken by circular to urge an extended trial of this remedy, and reports requested." The course usually adopted by the author was to give the arsenite of potash as in the following prescription:—Arsenite of potash and compound tincture of cardamoms, of each half a drachm; gum mucilage, three drachms; camphor mixture or water, half an ounce: mix. To be given every second hour four or five times, the last to anticipate the expected paroxysm at least two hours.—*Lancet, Aug.* 3, 1861, *p.* 112.

9.—ON ACTEA RACEMOSA IN ACUTE RHEUMATISM.

By Dr. Francis R. Mac Donald, Inveraray.

[Dr. Mac Donald thinks that this medicine is superior to all others in this tiresome disease. He says:]

This remedy is the tincture of the *Actea racemosa*, an American plant (natural order, *Ranunculaceæ.*) It was first brought under my notice by an accomplished physician, Dr. Voris, of New Rochelle, near New York, who was living for some time here two years ago. Dr. Voris spoke so strongly of the good effects of the Actea in rheu-

matism, that I immediately procured a supply. I have used it since in all the cases that have come under my notice. I was at first disposed to ascribe its beneficial effects less to its own virtues than to that coincidence of favourable circumstances which has frequently been observed to attend the use of a new medicine. Its uniform success has induced me to alter this opinion; and I believe that a similar success will be found to attend its employment in the vast majority of cases of acute rheumatism.

I have employed the Actea with equal benefit in all the acute forms of rheumatism. In the subacute variety already alluded to, it acts with remarkable readiness. There are not a few persons to be met with, either of a favouring diathesis. or living in rainy districts, who, in consequence of such an exposure as would elsewhere or otherwise lead to no greater inconvenience than a slight cold, are suddenly seized with rheumatic pains. In these cases the Actea is alike expeditious and satisfactory. But it is in the severest forms that its beneficial effects are best seen. Two well-developed cases came under my observation at the same time in last spring—one in a boy of nine, and the other in a man of forty. My supply of the Actea being exhausted, I was obliged to have recourse to the ancient treatment. In neither case did the disorder yield until the Actea had been given. The dose that I give varies from 25 to 40 drops, administered three times a-day.

The tincture of the Actea racemosa may be had of Messrs. Duncan and Flockhart, of Edinburgh, at 6d. per ounce.—*Edinburgh Monthly Journal, Oct.* 1861, *p.* 335.

10.—CASE OF ACUTE RHEUMATISM, WITH UNUSUAL COMPLICATION.

By Prof. Reid.

[At a meeting of the Belfast Clinical and Pathological Society, Prof. Reid related the following case:]

A young gentleman, aged 16, enjoying a comfortable home, was attacked for the second time with acute rheumatism. It began in the left ancle, and successively involved the knees, the elbows, and the wrists. A careful examination of the heart did not detect any evidence of disease as the result of the attack he had suffered from six years previously.

The treatment adopted at first consisted of alkaline aperients, alkaline anodyne lotions, a combination of calomel, squills, and opium; and enveloping some of the joints in cotton wool. For the first four days his bowels were very difficult to act on, so that he took a very large amount of Rochelle salts without their producing anything like a proportionate aperient effect. I thought afterwards that this might have had some influence in producing a complication I

had never met with before in acute rheumatism, and which has caused me to think the case worthy of record.

On the seventh day of his illness there appeared an ecchymosed patch or blotch, of the size of the hand, on the outer side of the right leg, not raised in the skin, nor removable by pressure; this was soon followed by similar patches on the arms, and by redness and great swelling of the left eye-lid, with effusion of blood beneath the conjunctiva. The right eye was soon involved in the same way; then the forehead and scalp swelled up into tumors the size of half an egg, producing, with his swollen face, a most disagreeable appearance.

The gums had become slightly swollen and coated from the mercury; on the day that the first discoloration was observed on the leg, the mercurial was at once discontinued, and an opiate and tonic treatment adopted. Salivation did not folllow, nor did the gums bleed at any time. There was no cough, nor oppression in the chest. The stomach now became very irritable, rejecting everything, and I became alarmed as to the result, and requested Professor Ferguson to see him.

He, like myself, had never met with a similar complication in acute rheumatism.

He suggested ten minims of diluted sulphuric acid every two hours to relieve the vomiting, which it did immediately, after the failure of full doses of opium, ether, and the aromatic sp. of ammonia. Tonics, opiates, and nourishment were continued.

At the end of four days the swelling of the face and forehead began to subside and the blood effused under the conjunctiva was slowly absorbed, and the discolorations of the skin gradually disappeared, as well as the affection of the joints. That these peculiar symptoms resulted from some change in the blood, permitting its transudation through the vessels, does not, I think, admit of any doubt. It then struck me, that possibly the alkaline solution, which had been taken so freely during the first few days of his disease, and which evidently had remained in the system, as it did not produce its usual aperient effect, was the cause of this altered condition of the blood, it being known that alkalies produce a more ready solubility of the fibrine, which, becoming more fluid and less coagulable, permits the blood to transude.

Believing that there is generally an excess of acid in the system in acute rheumatism, I have for many years past invariably administered alkaline aperients freely in the treatment of this disease, without their being followed by any such scorbutic symptoms as appeared in this case; but, then, I had never known three or four ounces of Rochelle salts to be taken daily for several days without producing free and continuous purgation.

It is, therefore, possible that the introduction and retention in the system of so much alkali might have injured the quality of the blood. For a few days after convalescence had commenced the evacuations

from the bowels were slimy and streaked with blood, but the latter never appeared in such a quantity as to indicate intestinal hemorrhage.

I was told, at my first visit, that his medical attendant in the former attack of acute rheumatism had stated that there was no heart complication. As I saw him within twelve hours after the commencement of the present one, I carefully examined the state of the heart, and found no evidence of present or past cardiac disease. Nor did a daily examination detect any till the morning of the fifth day, when a feeble murmur was heard at the apex, which continued feeble till the morning of the eleventh day of his rheumatism, when it was found to have become much louder.

For the first six days of the cardiac complication (during the two latter of which I had the advantage of Dr. Ferguson's opinion) no murmur could be heard through the solid whalebone stethoscope, though it was heard distinctly through the ordinary tubular one. On the seventh day it had become so loud that it could be heard through both, and continued to be so for five or six weeks, when it gradually subsided under the influence of blistering and free doses of hydriodate of potash.

The heart's action was easily excited for several months afterwards, and its impulse, on exertion or running, was so strong that it was often complained of.

This, however, gradually passed off, and a careful examination of the heart, made at the expiration of twelve months, from this last attack, failed to detect any evidence, or even suspicion, of disease, either at the valves or of the substance of the heart.

This young gentleman had scarcely become convalescent before his younger brother was attacked also with acute rheumatism, for the first time.

He was treated, at the commencement, with alkalies and moderate doses of mercury, care being taken that free purgation should take place.

He also suffered from a cardiac complication, which pursued much the same course as that in his brother, and left no permanent injury in the heart.

Fortunately, he was not attacked with scorbutic discoloration of the skin, nor with the swelling of the face or irritability of the stomach which had excited so much uneasiness and alarm during the treatment of his brother. His brother's complication should not, however, cause us to abandon the administration of alkalies during the treatment of acute rheumatism, but only inculcates the necessity of our insuring their non-retention in too large amount in the system by a combination of purgatives with them. Indeed the recent experiments of Dr. Richardson, which proved that he could at will produce inflammation in the interior of the heart, by the injection of lactic acid into the peritoneal cavities of dogs, placed the alkaline treatment

of acute rheumatism on a much more legitimate basis, by giving additional weight to the inference, that the cause of the disease and its cardiac complications was an excess of acid in the system.—*Dublin Hospital Gazette, June* 15, 1861, *p.* 177.

11.—*Medicinal Treatment of Gout.*—The combination which M. Trousseau conceives to be the most efficient is that of sulphate of quinine, colchicum, and digitalis, proposed by M. Becquerel in the following proportions:—Quinæ sulphatis, 22 grains; ext. semin. colchici, 8 grains; ext. digitalis, 4 grains. M. Divide in pilulas decem. Two or three of these pills should be exhibited in the course of the twenty-four hours, for two, three, or four successive days. M. Trousseau has prescribed these pills himself and witnessed their exhibition by others, with sometimes wonderful success. He has found the excruciating pain of a genuine acute paroxysm yield in seven or eight hours, and the attack itself subside in two or three days. These are the pills the professor prescribes in the incipient stage of anomalous gout.—*Dublin Hospital Gazette, June* 1, 1861, *p.* 175.

DISEASES OF THE NERVOUS SYSTEM.

12.—ON ACUTE SOFTENING OF THE MEDULLA SPINALIS.

By Dr. Robert Mayne, Physician to the Adelaide Hospital.

One of the most remarkable circumstances connected with the history of tumours of the brain, is the fact, that they are very frequently latent in the early stages. Many cases of chronic tumours of the brain have been presented to the Pathological Society which attained considerable dimensions, without having caused, during their early growth, any serious disturbance of the patient's general health, or having given rise to any symptoms calculated to create a suspicion of their existence.

It seems to be a law, in the development of these tumours, that if the growth be slow, and the increase gradual, so as to allow the surrounding brain to accommodate itself to the presence and pressure of the tumour, such tumour may arrive at a large size without creating anything like special symptoms. At length, however, when the tumour has reached a certain bulk, the surrounding brain becomes softened, or inflamed, or congested, or compressed, and when we find symptoms of cerebral disease setting in with great severity, and running a rapid course, symptoms which are, however, referable rather to the condition of the brain than to that of the tumour.

For what reason it may be I cannot very well say, but the history of tumours in the *spinal marrow* is not as well known as the history

of tumours in the brain. There can be no doubt that the former class of tumours is infinitely more rare than the latter, and this, added to the difficulty which the pathologist always experiences, in removing the spinal cord for examination, may probably account for our imperfect knowledge of such cases.

The following case is one of some interest, illustrating, as it does, many circumstances connected with the pathology of the spinal marrow.

A. B., twenty-one years of age, had always enjoyed good health previous to his last illness, with the exception of one brief rheumatic attack in his eighteenth year. He had been at work constantly and regularly up the period of his late attack. According to his own account, he was returning home after a hard day's work on the evening of the 10th of December last, when "all at once" he was seized with an acute pain "shooting down the left thigh and leg, and towards the ankle and foot." He was compelled to stop abruptly in the street, feeling unable to proceed; a short time after, he made an effort to walk on and he then experienced a similar "darting pain" throughout the entire length of the right limb, so that he could scarcely move. With much difficulty he managed to reach his home and get to bed, when he experienced a severe *rigor*. Subsequently he fell asleep and obtained some rest, but on awaking in the morning he was paralyzed in both lower limbs. The bladder was paralyzed from the commencement. He was received into St. Vincent's Hospital on the 12th of December, and placed under the care of Dr. Quinlan, who has obligingly furnished me with an abstract of his notes of the case whilst it remained under his care. All power of voluntary motion in the lower extremities was gone, and there seemed to be complete destruction of the excito-motory phenomena as well. The tactile sensibility of the paralyzed limbs was much impaired, but not quite destroyed. There was complete retention of urine. Doctor Quinlan considered the case to be one of acute softening of the spinal marrow, and, indeed, all the phenomena of the case seemed to confirm this opinion. The integuments over the sacral region soon became livid and sloughed, and the urine quickly became ammoniacal. By degrees the paralysis extended upwards, and at length the left upper extremity lost its power of motion, in great measure, and its tactile sensibility partially. All that medical skill could devise was done for the patient's relief, but with little avail. After being about a month in hospital, sloughing of the integuments set in wherever the body was subject to the least pressure. At about this period. the friends of the patient removed him from St. Vincent's Hospital, and shortly afterwards I found him in the hospital of the South Dublin Union. It is needless to recount the symptoms which he then presented, as they were the ordinary symptoms of complete paraplegia. There was fearful sloughing of the integuments over the sacral region and hips. The trochanters at each side, and the osseous structure of the sacrum, were laid

as bare as if they had been dissected, yet in the midst of this frightful destruction of parts, with this process of death going on in the living body, the patient seemed not to suffer any acute pain, and appeared perfectly unconscious of his terrible condition. All power being lost over the bowels and bladder, the fæces passed involuntarily and insensibly; an insufferable odour pervaded the atmosphere around him; the left upper extremity was completely paralyzed, the right was partially so; and altogether I have seldom witnessed a more distressing case. The man died on the 12th of February.

There was considerable difficulty experienced in obtaining a *post-mortem* examination, and, therefore, the state of the brain was not ascertained.

On laying open the vertebral canal, the "*theca vertebralis*" seemed healthy at the exterior, its inner surface appeared congested, and of a pinkish hue. It was only when the "pia mater" was slit up, that we discovered the diseased condition of the "*medulla spinalis*," and the extent to which the disorganization had reached. The diseased portion extended throughout the entire dorsal region, and was characterized by a sort of inflammatory *ramollissement*, reducing the ordinary firm texture of the medulla to the consistence and similitude of whipped cream, inflammatory action having tinged it of a red colour. But the most striking pathological phenomenon presented in the diseased parts, was a tumour of about the size of a small kidney bean, and oblong in shape, embedded in the substance of the "*medulla oblongata.*" When carefully examined it felt hard and firm to the touch, and *it cut like a piece of cartilage.* It was embedded in the left lateral half of the medulla oblongata. It seems strange that so dense a tumour could have grown in a portion of the nervous system so directly essential to life—a part in which the slightest hemorrhage causes immediately fatal results, and in which the slightest injury is mortal. The slow and gradual development of the tumour would, perhaps, account for this. We must, I think, believe, that the existence and growth of this tumour was the cause of the ramollisement; but if so, was it not strange that the softening of the spinal marrow began from below and extended upwards, thus producing the long protracted series of morbid results which terminated in the death of the patient?—*Dub. Hospital Gazette, May* 15, 1861, *p.* 145.

13.—ON REFLEX PARAPLEGIA.

By the Editor of the British Medical Journal.

[Perhaps one of the earliest and ablest writers on reflex paraplegia is Dr. Graves, in his lectures on paraplegia (1843).]

Dr. Brown-Séquard is, however, the latest writer on the subject. He gives us an excellent account of the affection, and also a novel explanation of its nature—an explanation, however, which we fear must,

for the present, remain hypothetical. Let us see what it is that he tells us on this important subject.

The two most frequently observed and clearly defined forms of paraplegia are, reflex paraplegia, and paralysis depending upon myelitis or disease of the spinal cord. Heretofore there has been no proper line of distinction drawn by pathologists between these two varieties of paralysis; but this distinction is absolutely necessary for the important purposes of rightly directing the practitioner's treatment; medicines which are suitable in the one kind being injurious in the other.

Paraplegia depending upon disease or injury of the spinal cord has been long well understood by us all; but what is meant by *reflex paraplegia?* Is there really such a disease? By reflex paraplegia is meant that kind of paralysis of the lower limbs which results from an excitation of the spinal cord, the excitation being conveyed to the spine through a sensory nerve. For example, some part of the body, say the uterus, is irritated, injured, or displaced; the effects of the displacement, &c., are appreciated by sensory nerves, and are thereby conveyed to the spinal nervous centre, and, by the action which they there produce, cause those changes to occur which result in paraplegia. According to Dr. Brown-Séquard, the paralysis is produced in the following way. The excitation aforesaid acts either upon the blood-vessels of the spinal cord itself, or on those of the motor nerves proceeding from it, or upon those of the muscle. This is what, pathologically speaking, is meant by reflex paraplegia. Through this excitation, spasmodic contraction of the blood-vessels is produced, and consequently a deficient supply of blood is sent to one or other of the aforesaid parts.

Dr. Brown-Séquard has, however, first of all, before proceeding to speak of its treatment, &c., to answer the objections of those who deny the existence of the affection; and there are a few men of high repute who do deny the thing. The answer of Dr. Brown-Séquard is, on the face of it, the best which can be given. He leaves transcendental physiology, and asks his readers to look at a plain series of facts which he unrolls before their eyes, and which are comprehensible to the understanding of the youngest and the oldest of us. To us, we must confess, these facts demonstrative are as "plenty as blackberries;" and we cannot imagine that any one except a *tête-montée* physiologist, or an absolute specialist can refuse to accept the consequences which flow from them. A female suffers from paraplegia. Her uterus is displaced. The abnormal position of it is rectified, and the paralysis disappears. Amaurosis exists in combination with irritation of the frontal nerve; the frontal nerve is divided, reflex action from the irritated spot is cut off, and sight rapidly returns. Neuralgia of external parts exists; and the patient's auditory powers are good, bad, and indifferent, according to the absence, presence, and acuity of the neuralgia. Dentition, worms, irritation of the womb,

diphtheritis, &c., are at times associated with various kinds of partial paralysis; and *sublatâ causâ tollitur effectus* has been duly observed in such cases.

Now, the proof that these paralyses do not depend upon disease of the nervous centres is this very plain one; viz., that the removal of the peripheric irritation—the worms, the displaced womb, &c.—could not in such case have removed the paralysis. And if a very hardened sceptic in these matters should still object that, nevertheless, the cures in such cases might have been more properly called rather a coincidence than a consequence, we should advise him to consult Dr. Brown-Séquard's book, and study his witnesses. The collection of cases there to be found, derived from numerous different sources, will, we fancy, demonstrate to the satisfaction of every one—except of those who, apparently for argument's sake, will on occasion deny that B follows A in the alphabet—that *reflex paralysis* is a medical fact.

In these cases it will be seen, that what Dr. Brown-Séquard calls the *outside irritation* preceded the paraplegia, and that cure of the latter quickly followed the removal of the irritation. Again, in many of these cases, the paralysis increases and decreases with the condition of acuity of the outside irritation; and no treatment has any effect in removing the paralysis so long as the irritation was left unremoved. In some of the cases the paraplegia appeared, disappeared, and reappeared several times over with the appearance, disappearance, and reappearance of the outside irritation. "Can there," says Dr. Brown-Séquard, "be more decisive proofs that it is the outside irritation starting from some sensitive nerves in various parts of the body, which produces the paraplegia?

Besides, if we do not accept this view of the case, we are left without any means of explanation of the paralysis in most of these cases. The idea of these kinds of paralysis being produced by, for instance, pressure of the uterus on the obturator nerves or on the sacral plexus; or by the action of poisonous matters accumulating in the blood in retention of urine, or in diphtheritis, or enteritis, is very chimerical.

But, it is objected, we cannot accept this reflex paraplegia; for it is impossible to understand how it can occur. And here the physiologist steps in, and answers without hesitation—*les pieces à la main*—that, on the contrary, it is very easy to show how a paralysis can take place by a reflex action starting from a sensitive nerve. And practitioners who pride themselves on being practical men, and *not* physiological physicians, must, we suppose, here take a lesson from the physiologist.

An outside irritation may produce paraplegia by *causing reflex contraction of blood-vessels* in two ways. It is proved beyond doubt, that blood-vessels have similar relations to the nervous system, to those which muscles of animal life have to this system. Blood-

vessels contract with energy and sometimes pass into a state of prolonged spasm: 1. when their motor nerves are *directly* excited; and 2. when excited through the cerebro-spinal axis through irritation applied to some centripetal or excito-motor nerve. In *these contractions of blood-vessels* we find the cause of paralysis. The contractions may occur: 1. In the blood-vessels of the spinal cord; 2. In the vessels of the motor nerve passing from the cord; and 3. In the vessels of the muscles themselves.

Dr. Brown-Séquard has, he tells us, seen contraction of the blood-vessels of the pia mater take place in the spinal cord, when the lobes of the kidney, or the vessels and nerves of the suprarenal capsules, were ligatured; and the contraction is generally greatest on that side of the cord corresponding with the side irritated. Paralysis of the lower limb has been known to follow extirpation of the kidney on the same side of the body. Again, reflex paraplegia may be produced by *insufficiency of nutrition*, resulting from this contraction of the blood-vessels. This explanation of the paraplegia as being a consequence of contraction of blood-vessels, must, however, at present be received as theoretical.

Another argument in favour of this view of the case is derived from the fact of the normal condition of the spinal cord—the absence of all visible signs of disease—observed in persons who have died after presenting symptoms of reflex paraplegia; as in cases of paraplegia associated with retention of urine.

Thus, then, here stand the facts touching Reflex Paraplegia:—1. There is no organic disease of spinal cord; 2. The paralysis will rapidly disappear when the source of reflex irritation is cut off; and 3. The vascular supply of cord, of motor nerves, and of muscles is under the influence of reflex action.

And now, then, we see the grand anatomical distinction between reflex paralysis, and paralysis associated with myelitis. In the first case, there is diminished, in the second, increased vascularity of the cord. In myelitis, the colour, the consistency, and the vascularity of the cord, are visibly changed. In reflex paraplegia, these qualities of the cord are found, to all appearance, unaltered after death. And here, therefore, as Dr. Brown-Séquard tells us, we have at once before us two groups of paraplegic diseases—one in which too much blood is circulating in the spinal cord, and one in which the very opposite condition of the cord exists. And then, he adds, consider the value of a right discrimination between them. The remedies which suit one group are actually injurious to the other. There are some, and he says that he can prove the fact, that diminish the amount of blood in the spinal cord; and there are others that increase the quantity. And yet, these remedies are often blindly and indiscriminately employed in paraplegia. Mercury, ergot of rye, and belladonna, Dr. Brown-Séquard asserts, diminish the blood, and strychnine and brucia increase the blood in the spinal cord.

[In most cases, reflex paraplegia may be characterised as follows:]

There is an absence of the special symptoms which indicate organic disease of the spine; the paralysis is incomplete, and has slowly supervened upon an affection of an urinary, a genital, or some abdominal organ, upon inflammation of the lungs or pleura, or after irritation of a nerve in its trunk or cutaneous ramifications. In most cases of paraplegia, we must form our judgment as to its nature from a consideration of these conditions.

[The following are the distinctions which Dr. Brown-Séquard has collected together and given as characteristic of the two best marked varieties of reflex and centric paraplegia—urinary paraplegia and myelitic paraplegia.]

Urinary paraplegia is *preceded* by an affection of the urinary organs. The paralysis is incomplete, and varies in degree with the varying conditions of these organs. The lower limbs alone are usually affected, and the paralysis does not extend upwards. Reflex power is not wholly lost or is much exalted. Rarely do spasms seize the paralysed muscles; nor is there pain in the spine, nor sense of contriction of the abdomen; neither formication, nor pricking, nor anæsthesia. Rarely is the bladder or rectum paralysed; there is much gastric disturbance; and lastly, recovery often takes place rapidly, after notable amelioration of the urinary affection.

On the other hand, in *myelitic paraplegia*, the affection of the urinary organs does not *precede*, but is a *consequence* of, the spinal disease. The paralysis is complete, and is unaffected by changes in the condition of the urinary organs. Other parts besides the lower limbs are usually paralysed, and paralysis gradually extends upwards. Reflex power is often lost, but sometimes much increased. There are always spasms and some amount of pain; also formication, pricking, &c.; feeling of abdominal constriction, and almost always anæsthesia or numbness. There is no gastric disturbance. The disease usually slowly progresses to a fatal termination, and rarely ever is there complete recovery. Moreover, the urine in myelitis is alkaline; whilst in reflex paraplegia not depending upon urinary diseases it is usually natural.

In considering the *prognosis* of reflex paraplegia, we must, according to Dr. Brown-Séquard, reflect that there are two forms of the affection; that, in one of these forms, the paraplegia depends upon a defect in the nutrition of the spinal cord; and in the other, upon an alteration of nutrition in the muscles of the lower limbs. The first of these forms is almost always curable, provided the external irritating cause be removable. But, in the second case, recovery rarely takes place, even though the provoking cause of the paraplegia be removed; and the reason is, that in this form the muscles have become rapidly atrophied and altered. Hence as a matter of prog-

nosis, it is always important to know, in a case of reflex paraplegia, whether or not rapid atrophy of the muscles has occurred.

The *treatment*—or, to speak more correctly, the indications for treatment, of reflex paraplegia springs naturally from the pathology of the disease as here laid down. The first indication evidently is to remove the local irritation, the cause of the affection; and this necessarily involves the consideration of the treatment of nephritis, cystitis, enteritis, morbid states of the uterus, in fact, of all those morbid conditions with which the paraplegia may be associated. In carrying out this treatment, we must also endeavour to diminish the sensibility, or interrupt the nervous influence which passes from the diseased organ or nerve to the spinal cord, to paralyse the nervous sensation. For this purpose, says Dr. Brown-Séquard, no narcotising agent is more powerful than belladonna; but, in some cases, caution is required in its use. In disease of the urethra or prostate, one grain of the extract of belladonna in twenty drops of laudanum should be injected into the urethra, and washed away after half-an-hour or an hour's residence there. This treatment should be repeated every two or three days; thirty drops of laudanum alone being injected on the intervening days. In bladder-disease, the aforesaid solution of belladonna is to be injected after evacuation of the urine; and then the sole laudanum injection on alternate days. Suppositories of these two agents may also be employed.

If the irritation radiate from the vagina or uterus, then a pill of half a grain of belladonna, with one grain of extract of opium, enclosed in a piece of cotton wool, may be passed well up into the vagina—a practice of M. Trousseau's, which Dr. Brown-Séquard much extols. Should the stomach, or the intestines, or kidney, &c., be the seat of local irritation, then opium should be taken by the mouth.

Dr. Graves has strongly recommended excitants or revulsives to be applied to the skin of the legs; and Dr. Brown-Séquard gives a theoretical explanation of their beneficial mode of action. They produce a *similia similibus* effect. We have seen that the disease, according to Dr. Brown-Séquard, results from a contraction of the smaller blood-vessels; but if this contraction be considerable, the muscular fibres of the vessels become exhausted, and then (by a well-established physiological law) they are relaxed, and so there follow dilatation of blood-vessels and admission of due flow of blood to the spine. Now, no irritating cause is more powerful in effecting contraction of blood-vessels by reflex action than cold; and, consequently, this sort of excitation, alternated with heat, should be applied to the spine. But, whatever be the kind of excitation employed in this case, it ought to be very powerful, in order to produce that exceeding degree of contractility of muscular fibres which is found, experimentally, to be followed by exhaustion and dilatation.

We fancy that to the theoretical explanation given by Dr. Brown-

Séquard (for we must still call it such) of the cause of reflex paraplegia, our readers will not at present be prepared to give their unqualified adhesion. To us, we must say that a tonic permanent spasm of the blood-vessels, such as is supposed by him to be the permanent state of the blood-vessels of some of the nerves or of the spinal cord in reflex paraplegia, is quite incomprehensible; and we think that on this point, which is assuredly the great point of Dr. Séquard's labours in this direction, he would have done well to have more fully developed the basis of his conclusion. Surely it is not enough to satisfy us on this head, that Dr. Brown-Sequard *has seen* a contraction of the blood-vessels in the spinal cord when "a tightened ligature was placed around the hilus of a kidney." *Permanent* contractions of blood-vessels of this character are surely contrary to what experience teaches us in other cases; and, if they really exist, how comes it that the cord does not become organically affected through such a cutting off of its nutritive supply?

Keeping this idea of the cause of the paraplegia in view, Dr. Brown-Séquard recommends that the patient, during the night, and, as far as may be, during the day, should lie in that position which would favour (by gravity) the flow of blood to the spine. As regards general treatment, this should consist in tonics and generous food; the partially paralysed muscles being as much exercised as possible in the open air.

Strychnia, Dr. Brown-Sequard says, is of the greatest use in this affection. It is the only remedy which really deserves confidence. It increases the amount of blood in the spinal cord, and (acting in a special and direct manner on the tissue of the cord) it increases the reflex faculty; the amount of blood and of the reflex faculty being, as propounded, below par in these cases. It may be given with opium in doses of one-thirtieth to one-fortieth of a grain a-day, and, when used alone, in daily doses of one-twentieth of a grain. When employed with belladonna its dose must be larger, to compete with the antagonistic action of the belladonna. And hot and ice-cold douches may be applied to the spine alternately, to produce that violent degree of spasm of the blood-vessels which is followed by their relaxation.

Besides this, the muscles of the paralysed limbs should be duly exercised, to prevent their becoming wasted and atrophied. It is therefore important to have them galvanised two or three times a week, and the limbs properly shampooed. If necessary, also, the heat of the limbs must be artificially kept up; remembering always that, unless the *primum mobile* of the paralysis—the local irritation—be removed, we can expect no permanent cure.—*British Medical Journal, April* 27 *and May* 25, 1861, *pp.* 437, 553.

14.—SOME REMARKS ON PARALYSIS.

By Dr. C. E. Brown-Sequard, F.R.S., Physician to the National Hospital for the Paralysed and Epileptic, &c.

[Paralysis is often caused by the irritation of a centripetal nerve, and yet this cause is not often noticed.]

The eye, which is the principal theatre of reflex action in the human body, is surrounded by muscles which are not unfrequently paralysed by a reflex action. In cases of neuralgia of the face, even when caused by a wound, a paralysis of the whole or of a branch of the third pair of nerves is often observed. This paralytic affection is generally easily cured when the neuralgia is cured.

The arms may be paralysed by a reflex action from various sources. In one case, after a sprain of the left elbow-joint, the whole of that arm from the shoulder to the elbow became paralysed, and a few days afterwards the right arm was also attacked with paralysis, and to a greater degree than the left. There was no other symptom of disease of the nervous system; nor was there any appearance of a rheumatic affection. For several months a variable degree of pain remained in the left elbow-joint, and many times during that period it was ascertained that the degree of paralysis was in correspondence with the degree of pain; and when the pain ceased altogether, the paralysis was soon completely cured. It will easily be admitted that I observed this case with interest and care, as I myself was the subject of the observation.

In the above case there was paralysis without wasting; it is not rare to find wasting accompany the paralysis when its origin is in some irritation of centripetal nerves. As regards the lower limbs, I have related elsewhere several such cases; as regards the arms, I have seen three cases in which an irritation from a wound on the fore-arm produced a reflex wasting palsy, either in the same arm (in parts the nerves of which had not been wounded), or in the other arm. I showed some time ago one of these cases at the London Medical Society, and Mr. W. Adams then said that he had seen a similar case.

The upper as well as the lower limbs, and other parts of the body, may be paralysed in consequence of an irritation of the bowels by worms. Moll, of Vienna, relates a case of paralysis of the two upper extremities, which had lasted three months, when it was suddenly cured after the expulsion of a very long tænia. In a case reported by Dr. Calvert Holland there were anæsthesia and paralysis of the lower extremities, and partially, also, of the upper extremities. No good effect was obtained from calomel, bleeding, and purging; while cure took place in two days after the expulsion of lumbrici.

Hemiplegia, also, is often caused by an external irritation. It is not rare in children, in consequence of an irritation of the dental nerves or of the bowels. It is not rare, also, in women at the time of parturition, or in other cases of irritation of the womb. Mangon,

quoted by Davaine, reports a very important case of a boy, nine years old, who was attacked with loss of consciousness, syncope, vomiting, and convulsions, and who, on recovering consciousness, was paralysed on the right side of the body. In a few days more than seventy lumbrici were expelled, and the patient gradually became better, and was cured of his paralysis in twelve days.

A paralysis of the face is often caused by an external irritation, such as a draught of cold air or a neuralgia. In a housemaid, who had frequent attacks of neuralgic pains in the infra-orbitalis nerve, I found a paralysis of all the muscles, and anæsthesia on the same side of the face during the paroxysms and for some time afterwards. Notta mentions several cases of facial paralysis evidently caused by a neuralgia.

In cases of paralysis it is often a difficult question to decide which of the two following causes is the one that has produced the paralysis; either an alteration of the blood, or a reflex action. In the many cases of paralysis appearing during pregnancy or parturition, or some time after delivery, collected by Dr. F. Churchill, the most varied forms of paralysis were observed; but it is difficult to know whether they were due—1st, to an alteration of the blood, in consequence of anæmia, chlorosis, albuminuria, or uræmia; 2ndly, to a reflex action from the womb or vagina; or 3rdly, to a combination of these two causes. The same difficulty exists in cases of paralysis following diphtheria, typhus, rheumatism, gout, fever and ague, measles, scarlet fever, small-pox, dysentery, and diseases of the liver or the kidneys. I think that in most of these cases the two causes co-exist, the alteration of the blood producing the general debility, and the reflex action the greater local debility, which amounts to a paralysis. We generally find in those cases that the paralysis is in the neighbourhood of some part that has been the seat of pain—i. e., of an irritation of centripetal nerves, which is a fact in harmony with the view that it is at least partly caused by a reflex action. I may add that in cases of visceral inflammation, without any evident alteration of the blood, a paralysis may also be produced apparently by a reflex action only; so it is in cases of paralysis occurring during or after a gastro-intestinal inflammation, a bronchitis, a simple sore-throat, a pleurisy, a pneumonia, &c., when there is no reason whatever to suspect that some organic lesion exists in the nervous centres.

It is not my intention to explain by what mechanism a reflex paralysis is produced. My only object is to show that the most varied forms of paralysis may be due to an external irritation of some centripetal nerve.

Loss of Speech.—This symptom may be due to the most distinct causes; 1st, some peculiar affection of the mind; 2nd, a defect in the memory of words; 3rd, a paralysis of the muscles of the larynx; 4th, a paralysis of the tongue. Leaving aside the first two causes, we find that a paralysis of either the larynx or the tongue may be due, like

any other local paralysis, to some organic affection of the nervous centres or motor nerves, to some organic affection of the muscles, to an alteration of the blood, or to a reflex action. Loss of speech is a symptom induced more frequently than any other by a sympathetic influence from the cerebral lobes upon the basis of the brain.

Delirium and Insanity.—No one who has carefully studied these effects of certain disturbances of the nutrition of the brain can deny that the pretended organic causes of mental disorders are not their essential cause. It is impossible to admit that an alteration limited to a small part of the brain can be the cause of all the shades and degrees of delirium and of the numerous forms of insanity, and it is therefore necessary to conclude that when these various mental disorders arise from a limited organic affection of the brain, they are due to some peculiar influence exerted at a distance from the seat of the organic alteration upon other parts of the brain. As regards the production of insanity by a reflex action, facts are so abundant that it is scarcely requisite to mention any of them, as every one must have seen some. In the case of a gentleman brought to me by Mr. Foulkes, in December last, there was insanity with general paralysis, which I suspected to be caused by an inflammation of the prepuce and of the glans, with phymosis. According to my advice Mr. Foulkes performed circumcision, and the patient was soon cured of insanity. Shortly afterwards he improved rapidly as regards the paralysis, and he is now cured. This is a clear case of reflex insanity and reflex paralysis.

That the various forms of insanity and delirium may be caused by irritations of centripetal nerves, or by alterations of the blood, we have positive proofs every day in cases of worms or of any irritation of mucous membranes or of the skin, of acute inflammation of the thoracic or abdominal viscera, of chlorosis, of anæmia, of albuminuria, of poisoning by lead, arsenic, &c. As there cannot be the least doubt in this respect, I will not attempt to give a demonstration.

Coma.—This grave symptom is generally due to a pressure upon the brain by effused fluids (pus, blood, serosity). I need not say that hemorrhages, inflammations, and pressure upon sinuses or veins are frequent causes of the effusion to which coma is generally due. My object is simply to state that effusions producing coma are very often caused by irritations upon centripetal nerve-fibres in the different parts of the body, acting upon the membranes of the brain, in such a way as to produce a more or less acute inflammatory state, and the consequent purulent or serous effusions. In children, indeed, such a thing is extremely frequent; it is not rare also in adults in cases of fever, especially with disease of the abdominal or thoracic viscera. Coma is then the effect of a reflex action. It seems probable that an irritation from certain parts of the cerebral lobes does sometimes act in the same way as the peripheric irritations or visceral nerves of the thorax and abdomen. Anæmia and several other affections with alteration of the blood, are sometimes also the causes of effusions producing coma.

Contraction and Rigidity.—These symptoms are so well known as being sometimes the effects of a reflex action (caused by worms, teething, &c.), that I have not to insist on that mode of their origin. I wish now only to say that if we admit that in cases of inflammation in a small part of the cerebral lobes, especially in their upper portion, a rigid or contractured state of paralysed muscles (a pretty constant symptom then) is due to the fact that the motor nerves of these muscles are irritated in the inflamed part, we should have to admit also this absurd, or at least impossible view, that the various motor nerves of the muscles of the limbs go up to each of the different parts of the cerebral lobes. We must admit that in those cases the rigidity or contracture is produced through some peculiar influence, exerted by the inflamed nerve-fibres in the cerebrum, upon some parts of the base of the brain. Is that peculiar influence a reflex influence? I cannot prove that it is; but I think the probability is great that there is then the same process that exists when contracture is due to a reflex action from the bowels, the gums, &c.

Convulsions and Convulsive Affections.—I need not give proofs that irritations starting from any part of the body often produce convulsions, or any form of the convulsive affections (epilepsy, hysteria, catalepsy, chorea, tetanus, and even hydrophobia). There cannot be the least doubt on this point, and facts are too numerous and too striking for my insisting upon this mode of origin of convulsions. I wish only to speak of convulsions or convulsive affections depending upon an organic disease of the cerebrum or the cerebellum. In what way does such a disease produce these symptoms or symptomatic affections? To answer the question, let us suppose, for instance, a case of a small tumour in the upper parts of the cerebellum (without pressure upon the base of the brain), producing epileptic fits. In such a case, no motor nerve-fibre going to the various muscles of the head, the trunk, and the limbs, is directly irritated by the tumour. The mode of action of the tumour must therefore be through some peculiar influence exerted over those parts of the base of the brain where exist the motor nerve-fibres. It is, in fact, by an action on a distant part of the nervous centres that the irritation in the cerebellum then acts, just as is the case when epileptic fits are the consequence of an irritation in the bowels, the skin, &c. The same thing may be said of epilepsy caused by a disease of the cerebral lobes. So that convulsions or convulsive affections symptomatic of disease located in the cerebral lobes or the cerebellum owe their origin to a sympathetic influence of the part altered upon other parts of the brain.

Conclusions.—I hope I have succeeded in rendering probable, if not certain,—

1st. That the principal symptoms, and the various affections symptomatic of brain disease, originate much less frequently than it is admitted from the loss of function of a part of the brain, or from some direct effect of a disturbance of that function.

2nd. That an alteration of the blood may be the cause of these symptoms and symptomatic affections.

3rd. That the diseases of the cerebral lobes, or of the cerebellum, give rise to symptoms and symptomatic affections chiefly through a peculiar influence exerted by the part altered upon other parts of the encephalon.

4th. That the principal symptoms and affections symptomatic of brain disease, may be caused by an irritation starting from any viscus or any nerve of the skin or mucous membranes.—*Lancet, July* 27, 1861, *p.* 79.

15.—TREATMENT OF DELIRIUM TREMENS BY LARGE DOSES OF TINCTURE OF DIGITALIS.

By Dr. Francis E. Carey, Guernsey.

As the treatment of delirium tremens by large doses of tinct. digitalis is still in its infancy, perhaps the narration of a few cases which have occurred in my practice since Mr. Jones's report, will not prove uninteresting. The hesitation which I at first entertained in prescribing these large doses, induced me to give our hitherto recognized sheet-anchor, opium, a fair chance; but it will be seen how egregiously its character was belied by the results, it being actually instrumental in aggravating the sensorial excitement, though no contra-indication existed to its administration.

P. B., aged 42, carpenter, was admitted into the Hospital February 26, 1861, with well-defined indications of delirium tremens. I was requested to visit him the previous day at his own house, and found him labouring under all the hallucinations of the disease consequent on a protracted debauch of some weeks, which had depressed and enervated a naturally feeble system. For nearly three weeks he had been unable to procure any refreshing sleep. During this day, I prescribed four grains of solid opium and ʒj. of tinct. opii, which produced the inverse effect of aggravating the cerebral excitement. The evening of the day of his admission, I administered a half-ounce dose of tinct. digitalis, with an equal quantity of gin, which, after the lapse of half an hour, caused him to fall into a deep sleep, which continued with one brief intermission for twelve hours. He awoke quite rational, and two days subsequently was presented cured. The digitalis did not produce its alleged results, diarrhœa and vomiting, nor were the kidneys stimulated to increased action.

R. C., aged 30, after a prolonged course of dissipation, was seized with unmistakeable symptoms of delirium tremens. On his removal to the Hospital, which was necessitated by his extreme violence, eighty drops of laudanum were given. At eight o'clock p.m., as there was no mitigation of the delirium, an additional dose of forty drops was ordered. The night was spent in wandering and in attempts to

escape from restraint. The following morning, after a copious evacuation, he was ordered a combination of laudanum (forty drops), and antimony, which was repeated after four hours. He had then taken the equivalent of at least ten grains of opium in twenty-four hours. In the evening, as no amelioration had taken place, the half-ounce dose of digitalis was administered, with an equal quantity of gin. His condition throughout the night was calmer, and for four hours he enjoyed undisturbed sleep. No further treatment was deemed necessary, notwithstanding that he continued somewhat disposed to be boisterous during the next twenty-four hours. A subsidence of this soon took place, and he was dismissed cured at the end of the fifth day from his admission.

A third case occurred in a gentleman, aged 28, whose nervous system had also been subjected to the effects of prolonged alcoholic drinking. Opium was equally generously administered, but, as in the preceding cases, its inefficiency was most manifest. Recourse was had to the digitalis with such good results, as to tranquillise our patient, who had been afflicted with the most soul-torturing delusions it has yet been my lot to witness.

The fourth case has occurred within the last few days in a man aged 25. He was the subject of epilepsy, induced by his habits. His appearance betokened serious disease of the heart, confirmed by auscultation, which detected an aortic systolic murmur, with extensive hypertrophy. This patient's daily consumption of raw spirit was not much below two or three pints. Aberration of mind quite characteristic. Opium was again resorted to, but its shortcomings were as evident as in the other cases. Half an ounce of digitalis was given in the morning. Notwithstanding my remonstrances, the patient left his room and wandered about the house, much quieted, however, till he retired to bed at his usual hour. Next morning, I found him calm and composed, having had a few hour's sleep. The immediate effect of the digitalis here, was to equallise the circulation, and to restore warmth to the extremities, chilled by his morning's walk.

This latter case is interesting, as indicative of the non-injurious effects of wholesale doses of digitalis in cases where morbus cordis exists. This condition of the heart, though not to the same extent, was detected in the preceding case. In any of these cases, should the first dose of the digitalis not have sufficed, I would not have scrupled administering a second, as the influence on the pulse was not such as to offer any barrier to its repetition. In proffering these remarks, I cannot avoid expressing deep satisfaction for the application of a drug to the cure of a disease, which, hitherto, has very frequently resisted the most heroic treatment. From my small experience I cannot but entertain the hope that the mortality, which has been due, without exaggeration, as much to the large doses of opium as to the unrestrained development of cerebral complications, will be strikingly diminished.—*Med. Times and Gazette, August* 24, 1861, *p.* 206.

16.—DIGITALIS IN DELIRIUM TREMENS.

By Dr. D. M. Williams, Liverpool.

[Dr. Williams gives us the following interesting case to illustrate this new mode of treatment.]

At noon of Wednesday, August 28, I was requested to see a private patient, whom I have cured of delirium tremens on two previous occasions by a single subcutaneous injection of ⅔rd grain of sulphate of morphia, dissolved in ℥ xx. of water, Since the last attack he had been perfectly sober, until August 13, when he began to drink, but continued to eat as usual until the 20th, from which time he drank brandy, and continued doing so up to the day I saw him. He became delirious on the 26th, and slept none during that or the following night, nor had he eaten food since the 20th.

When I saw him he was perfectly furious, almost naked, and struggling to get out of the house; with difficulty I ascertained that the pulse was 108. 2 p.m. Administered tinct. digitalis, ℥ss., and persuaded him into bed. In twenty minutes the pulse had fallen to 96, full and soft; tongue loaded. He was still very much excited, and I thought the lowering of the pulse was due to position and soothing conversation. To have another ℥ss. tinct. digit. at 4 o'clock. 5 p.m. An hour has elapsed since he took the second dose of the tincture; he has been extremely wild and restless, excepting once, when he seemed to fall into a quiet dose for a few minutes, but awoke as mad as ever. Pulse 96, full, soft, and regular; skin cool, but as he is almost naked, and cannot be induced to lie in bed covered, that is easily accounted for. To have another ℥ss. tinct. digit. at 7 o'clock. At 10.30 p.m. had become more manageable, and has been quieter since 6 o'clock; pulse 96, but wiry, with an uncertainty about an occasional beat; has slight nausea, will take no food, but wants beer continually; to have a little brandy and lemonade during the night, and as much beef-tea as he can be induced to take.

29th., 10.30 a.m. Has had a bad night, has not slept more than an hour and a-half since he took the first dose, but has been vomiting and purging very much all night, with violent palpitation of the heart between 6 and 10 o'clock this morning. He attempted to take beef-tea, also brandy and lemonade, but vomited everything; he is, however, quite quiet, talks rationally, and complains much of the palpitation. Pulse 86, full, soft, and steady; skin comfortably warm and moist, tongue cleaner; to take ℥ss. every three hours of the following mixture, and as much food as possible:—℞. Bismuth subcarb. ʒij., sol. morph. ℥j., æther chlor. ℥jss., mucilage Arab. ℥ss., aquæ ad ℥viij., M. 10 p.m. Has spent the day at Eastham, where he took six and a-half glasses of brandy, but no food or medicine. Has had no vomiting or purging since morning; kidneys have acted throughout as usual. He is now quite quiet, but provokingly obstinate, and craving more brandy; pulse 96. All stimulants forbidden; to take

his mixture and as much milk, beef-tea, &c., as possible during the night.

30th. He is so much better of his delirium that his friends allowed him to go to Seaforth alone, as he complained of being ill, and thought a little change would enable him to return to business quicker. His wife says he has had a good night, and she thinks he slept a good deal, has had no stimulants, but drank a large quantity of milk, and from this time steadily recovered.—*Med. Times and Gazette, Sept.* 14, 1861, *p.* 284.

17.—DELIRIUM TREMENS TREATED BY LARGE DOSES OF TINCTURE OF DIGITALIS.

By Dr. Thomas B. Peacock, Physician to St. Thomas's Hospital, &c.

[The following account of the use of this remedy in delirium tremens adds to the interest of the preceding papers on the same subject.]

The cases of delirium tremens in which the large doses of tincture of digitalis were used at St. Thomas's Hospital, have been six in number (three only included in the former enumeration), five of these being cases of idiopathic delirium, in which the affection was the direct result of excessive drinking, while in the sixth the disease was traumatic, or supervened in a man of dissipated habits, shortly after he had sustained a fracture of the thigh.

In four of these cases only one dose of half-an-ounce was given; in a fifth, the same quantity was repeated after an interval of twenty-two hours; and in the sixth, the dose first given was only two drachms, and half-an-ounce was exhibited after an interval of about twenty-four hours. In two of five cases of which I possess notes, the stronger alcoholic stimulants were given in the earlier periods of the attacks, and in all but one or two, ammonia and bark were prescribed after the digitalis had produced its effects. The ages of the patients treated were 23, 30, 33, 44, and 45. The last patient only was a female, and all recovered.

In no instance were any alarming symptoms of depression induced; but the pulse usually soon fell remarkably in frequency, and acquired greater power and steadiness. The sickness subsided; the tremor became less, and the patient was much quieted. It did not, however, appear that the digitalis directly induced sleep, but it calmed the patient, and sleep seemed to ensue naturally, after a longer or shorter interval. The patient did not, however, sleep so soon, so soundly, or for so long a period, as, from Mr. Jones' report, had been anticipated; and in two cases morphia was given before rest was procured; while, in a third, small doses of compound ipecacuan powder were exhibited at short intervals during the attack. This difference may be due to the less liberal employment of the remedy at St. Thomas's. Mr.

Jones states that he generally repeats the dose in four hours, and occasionally, in a smaller quantity, for a second time; whereas at St. Thomas's two full doses were only prescribed in one case, and in that instance the second was given after an entire day had elapsed.

The kidneys did not appear to have their action increased by the digitalis; on the contrary, so far as could be ascertained, the flow of urine continued, after the remedy was exhibited, to be only small in quantity.

I would remark that it would be altogether premature to venture a decisive opinion upon the use of the large doses of digitalis in this disease; but the facts seem to warrant the conclusions—

1st. That the drug, when exhibited in full doses, does not by any means produce the amount of depression which our previous experience of its action, in small and frequently-repeated doses, would have led us to expect; and

2nd. That the remedy, in conjunction with other means, may probably be very usefully employed in the treatment of certain cases of the disease, and especially when it occurs in young and robust persons, whose strength has not been broken down by prolonged habits of intemperance; and particularly when it arises as the immediate result of excessive spirit drinking. Delirium tremens, like other acute diseases, occurs under varied and opposite conditions, and it would be alike opposed to sound theory and practice to extend the same treatment to all cases of the disease.

I have selected two cases to illustrate the views here expressed,—the first is one in which the disease occurred after excessive drinking; in the second, a dissipated man became delirious after having sustained a fracture of the thigh, from being deprived of his accustomed stimulus.

Case 1.—J. T., aged 30, formerly a soldier who had served in North America, the Crimea, and India, and had just returned from Calcutta, was admitted into St. Thomas's Hospital, under my care, on October 14, 1860.

He was received, labouring under symptoms of delirium tremens, at half-past two, p.m., and was reported to have taken a quart of spirits daily for several days. At half-past six, when he was in a very restless and excited state, suffering from retching and sickness, and his pulse 103 in the minute, he was directed to have half-an-ounce of tincture of digatilis, and the dose was given in a quarter of an hour. At a quarter to eight the pulse had fallen to 72, and was of good volume, and he was much quieter, free from sickness, and disposed to sleep. At eleven p.m., he was much in the same state; pulse 76 and regular; he had not slept, but was quiet and still seemed disposed to sleep.

At ten a.m. on the 15th he was seen by myself: he was reported not to have slept ten minutes during the night, and had been restless and trying to get out of bed, but not violent. The tongue was dry and

a little red, the pulse 80, somewhat sharp, and decidedly resistent. The sounds of the heart were natural, except the second, which was somewhat indistinct. The pupils were large, and not apparently affected by light. The urine deficient in quantity, and highly coloured. His manner was hasty, but he spoke quite rationally, and he took his food well. During the evening and night of the same day he slept for a considerable period, and the following morning he was much better. He was quite intelligent, but still had a somewhat agitated manner. The pulse was 80 to 84 in the minute and feeble, and the second sound of the heart was very indistinctly audible. He was directed to have five grains of the sesquicarbonate of ammonia in an ounce and a-half of decoction of bark every three hours.

On the 17th he continued to improve, and on the 18th his appearance and manner were much more tranquil, the tongue somewhat dry and slightly furred: the bowels regular; pulse 80. He had made water freely, but said that the quantity was less than usual. He was directed to have the quinine and iron mixture three times daily and the mixed diet. To be allowed to leave his bed. When up there was a considerable increase in the frequency of the pulse—greater than was perhaps explained by his general state—in bed it beat steadily 80 in the minute, when sitting up it was 100, and was more irritable and feeble, and when standing it became 120, very sharp and feeble.

He was presented and went out cured on the 20th.

During the period of his residence in the hospital he was not allowed any stimulants.

Case 2.—S. S., aged 44, a waterman, admitted into St. Thomas's Hospital, on October 15, 1860, under the care of Mr. Mc Murdo, after having sustained a fracture of the thigh from a fall. On the 18th he began to have symptoms of delirium tremens, and had become very violent when, at 4.45 p.m., a dose of 40m\ of tincture opii was given to him. At 10.30 p.m., the laudanum was found not to have had any soothing effect, and his pulse was 120, and ℥ss. of tincture of digitalis was given. At 11.15 p.m. the pulse was still 120.

19th. At 9 a.m. he was reported to have been violent during the night, but less after taking the dose of digitalis till 4 a.m., when he again became more excited. He had no sleep during the night, but was then quieter. Pulse 98, quiet, and feeble. Four p.m. He had still had no sleep. The pulse was 98, full, and regular. To repeat the dose of tincture digitalis. Ten p.m. Has still not slept. Pulse 82, full, and regular. Pupils natural and contractile. He has passed a small quantity of water. He appears disposed to sleep.

20th. 4.30 a.m. Has still not slept. Pulse 80, regular. He has passed some water, which smells of digitalis. To have a glass of gin. 11.25 a.m. Still no sleep. Pulse 80, irregular, and intermittent. 4.45 p.m. I saw him with Mr. Whitfield. He had still had no sleep. The pulse was 80 and irregular, and he was much depressed. To have

6 grains of the sesquicarbonate of ammonia in an ounce of decoction of bark every four hours. Two pints of porter in 24 hours. Nine p.m. Pulse 70, irregular, and intermittent. The bowels have been acted upon. To have four ounces of gin and half-a-drachm of the solution of hydrochlorate of morphia and a drachm of aromatic spirit of ammonia directly. Eleven p.m. Pulse 88, regular.

21st, 10 a.m. Did not sleep till five o'clock this morning. The pulse is now 84, full, soft, regular. The tongue is dry, and he complains of thirst. He has passed water naturally. Nine p.m. Has slept at intervals and taken his meals naturally, is otherwise going on well.

22nd, 10 a.m. Has slept during the night. Pulse 80, soft, slightly intermittent. The sesquicarbonate of ammonia every six hours. The full diet. From this time he progressed favourably.

Note.—At the time when the large doses of digitalis were first tried in the Hospital, struck by the very little effect which resulted from the treatment, I suggested to Mr. Whitfield the employment of digitaline; as MM. Homolle and Quevenne, and the reporters on their communication to the Academie de Médecine,—Rayer, Soubeiran, and Bouillaud,—had shown that while capable of producing all the effects of digitalis, the digitaline was much more certain in its action. This suggestion was adopted, and a case was successfully treated by a dose of the active principle regarded as equivalent to the large doses of digitalis, but I have failed to obtain the notes of this case.—*Med. Times and Gazette, Aug.* 3, 1861, *p.* 105.

DISEASES OF THE ORGANS OF CIRCULATION.

18.—ON BLOOD-LETTING IN APOPLEXY.

By Dr. Thomas K. Chambers, Physician to St. Mary's Hospital, &c.

[Perhaps no question is more serious than that regarding the propriety of taking blood in cases of coma, apoplexy, and paralysis. Delay may be irretrievable; yet, a false step may be equally fatal.]

In the first place, let us clearly understand what is the *modus operandi* of the means you design to employ, and then what morbid conditions it can and what it cannot relieve.

The two most marked immediate effects of blood-letting are, diminution of the force of the heart, and contraction of the area of the blood-vessels. That these are the most important and most direct of the resulting phenomena, is shown by their being in direct proportion to the dose; the more rapid and copious the detraction of blood, the more certainly they follow. Both natural and artificial hemorrhage

thus produce first syncope, or failure of heart-power; and then a slackening and arrest of the blood-stream by contraction of the vessels.

What now are the morbid conditions in apoplexy which such results as these are likely to benefit? I am not going to enumerate all the causes of apoplexy, because such a recapitulation would be very unsuited for the purpose of the present lecture, and, if they came into your head at the time you were in presence of your patient, would serve rather to confuse than to clear your ideas. A most practical thing to do will be to divide them into three heads with reference to the treatment to be pursued; and I think you will find them all capable of being classified under *destruction* of the nerve-fibres, *compression* of the same by a *solid*, and *compression by a fluid* substance.

Now, if the nerve-fibres be destroyed, as is the case, for example, in what appears after death as a softening of the cerebral substance, it seems unadvisable to take away the pabulum of nutrition; for you would be diminishing the power of the remaining brain-tissue to take the place, as far as it can, of that which is irretrievably lost. Nor are the immediate effects of blood-letting likely to be advantageous; for in these cases the force of the heart is not morbidly augmented, and the blood-stream is in general rather deficient in quantity already. We shall probably be only adding to the disease by subtracting blood. When, then, we have reason to think from the previous history that there is old disease leading to softening of the cerebral substance, we should abstain from bleeding altogether. If, for instance, there have been before the present attack a state of mental excitement, or severe headache, or feverishness; or if the gait have been unsteady, the speech thick and stammering; if there have been earache, or a purulent discharge from the auditory meatus; if previous to the apoplexy, there have been convulsions; if the patient be scrofulous, or of a scrofulous family,—we must take all these circumstance as warnings against the lancet—altogether, probably, but certainly against its immediate use.

Supposing, however, that we have no evidence of such a diseased state of the cerebral substance, and can allow ourselves to conjecture that the nerve-fibres are more compressed than permanently disorganised, then we have further to consider whether the compressing substance is *solid* or *fluid*. By a solid likely to cause apoplectic paralysis, I refer to a clot of blood which has been thrown out long enough to become entirely coagulated. This is the only solid sufficiently common as a cause for us to consider. Is it likely you have to deal with anything of this sort? Is there in the patient's brain before you at the moment a clot of blood such as you see put up in bottles in the museum, and often also in the dead-house? Do not be in too great a hurry to conclude so at once. Four days ago you saw on the dissecting-table the corpse of a man who more than thirty-six hours previously had fallen down stairs, and was supposed to have fractured

his skull: the cranium was uninjured, the fall was apoplectic, and there was a quantity of fluid blood pressing upon the brain. You see by such instances as these that blood extravasated from the vessels inside the body does not coagulate immediately, as it does when drawn from a vein; it remains long, and may remain much longer than it did in that case, in a liquid state. It takes, not minutes, not hours, but I may really say days, to become solid. When the apoplexy or paralysis has lasted for days, then indeed, but not till then, you may conclude that a clot has formed; and then, and not till then, may you pronounce that the force of the heart's action will not increase the oozing of blood into the focus of pressure. After that —after you have a solid, and not a fluid pressure, to deal with—I am willing to concede, and anxious to impress upon you, that abstraction of blood is useless and hurtful; but till then I believe its immediate influence to be a beneficial one, and in the right direction. Therefore do not conclude that a clot is formed, or that bleeding is for that reason useless, till at least twenty-four hours are passed after the fit.

Supposing that a fluid has to be dealt with, you are perhaps anxious to know of what nature it is; and, in point of fact, I find that I am constantly asked by students, when they see a case of apoplexy in the wards, whether it is serous or sanguineous apoplexy. I say to you now, what I always say then, that there is no possibility of certainly distinguishing them; and that, fortunately, it is of no importance to either physician or patient to do so, as far as the immediate treatment of the fit is concerned. One, indeed, forms a clot after some time, which we have been just now discussing; and the other remains liquid. But that does not affect the question of the comatose condition now before us. I will speak, then, of "fluid", not distinguishing blood or serum, or bloody serum or serous blood, but clubbing them all, as must be done by practical men, together. When, then, there is fluid compressing the nerve-fibres, I feel sure that there is an influence for good in blood-letting, accompanied, of course, by a danger, as all active treatment is, but still a decided influence for good. It is capable of lessening the force of the heart, which is driving the blood towards the place where it is oozing out either as serum or complete blood, and of diminishing the calibre of the vessels that allow it to pass. The anxious question is, how to secure those advantages without the necessarily accompanying dangars, without adding too great an additional shock to the already shocked nerves, without weakening fatally the already weakened general system.

You will observe, by the example of the patient I have used as my text, that the first effect of an apoplectic seizure is the violent blow to the nervous system above mentioned. The poor woman was damp and cold, and pale as a corpse; the pulse and heart beat quick, and irregularly, and weakly, just like those of an ox stunned by the slaughterer's pole-axe. Had she been bled then, she would certainly have died outright; and moreover, even had she survived, there was

no object to be gained by it; for the heart was weak enough, *surely*. But after a time—seven hours in this instance, sometimes sooner, sometimes later—the heart and pulse recover, and the blood must be driven against the ruptured or oozing vessels with the same force which originally caused them to rupture or ooze. Now is the time to step in with the lancet. You prevent by it what so often happens to apoplectic patients if left alone entirely; you prevent the relapse into coma which frequently follows an apparent partial recovery during reaction; you prevent it in a very intelligible way, by lowering the force of the blood-stream, which the injured vessels have already shown themselves unable to bear. This was the time, and this was the object, of the blood-letting in our patient's case. So complete was the apoplexy, by such a slender thread was she hanging on to life, that I believe the slightest additional extravasation of fluid in the cranium must have been fatal; and to this judicious use of the lancet by our house-surgeon, Mr. Chisholm, I attribute the patient's life.

You will observe the bleeding was not a large one—only eight ounces when reaction first occurred, and eight ounces again in the evening, when the pulse was again getting hard. I mention this to warn you against a mistake into which you might be led by an old and rather questionable maxim "*Extremis morbis, extrema remedia*". You might be disposed to say, the more the apoplexy, the more the bleeding. Such a notion would be most dangerous. A small bleeding accomplishes the object in view; and a large one can do no more, while it seriously abridges the vital powers. Above all things, don't open the temporal artery; it is nearly equivalent to cutting your patient's throat.

I much prefer, in these cases, venesection to cupping the temples or the nape of the neck, as is sometimes done. What you want to do is to affect the general system, and particularly the centre of circulation—an object which is attained with most rapidity and certainty by opening a vein. You are aiming to prevent blood from being driven into the skull, not to extract that which is already in: at least, if you are striving after the latter result, your anatomy and physiology ought to have taught you better.

The effect of bleeding during the collapse, before reaction has taken place, is, as a rule, sudden death, especially when the heart or its valves are diseased. The effects of *over-bleeding* at a later period are, an excited condition of the circulation, and consequently a more violent impetus against the brain; and at the same time a more watery state of the blood, and consequently a greater tendency to serous effusion. As it is impossible to tell whether red blood or serum is exuded in the cranium, you run the greatest possible risk of augmenting the very evil which your injudicious zeal was intended to obviate.

Perhaps you may say, "I am going to bleed my patient again, for the sake of encouraging absorption; and surely absorption of the clot should be aimed at". Now, I am not so sure of that: if it were

absorbed sooner than the nerve-fibres were ready to resume their functions, something else must be effused to take its place in the cranium. At all events, I am quite convinced that such absorption is best left to the reviving powers of nature; and you are diminishing these by unnecessarily depriving the body of blood.

In apoplexy and central paralysis, remember that you are dealing with diseases where nature still retains an inherent power of repair: in a moderately healthy constitution, the tendency is to get well. Do not, therefore, attribute all cases of recovery to the means used, unless you can give a probable physiological explanation of their beneficial action; but if you can thus rationally justify your treatment, do not hesitate to feel satisfied with it, whether successful or unsuccessful.

There is no necessity for being idle while you are awaiting the time to bleed. Stimulant injections and purgatives, hot water and mustary poultices to the feet and legs, a careful arrangement of the clothing and bedding so as to prevent congestion to the head, with keeping up the animal warmth and keeping the friends quiet, will amply occupy your time. It is also conducive to the end you have in view, to shave the head and apply cold lotions to the skull.

After reaction has returned, you will find advantage in the application of ice to the head; it is physiologically correct, and is often a great relief to the patient. You may remember that this poor woman used the first power of speaking that returned to her in begging me to continue the ice; whenever it was applied, it induced sleep, and was most agreeable. What a strange state a person must be in for ice to the head to be agreeable! But so it was.

When, then, you are called in to a case of apoplexy, let these considerations pass through your minds:—

1. Does it depend on destructive softening of the cerebral substance? If so, I must not let anything persuade me to bleed.

2. Has the effusion taken place so long ago as to be, if blood, coagulated? If so, again, I had better abstain.

3. If it be fluid, bleeding is very much to be desired; it may prevent increase of the effusion, and relapse.

4. But to make bleeding most useful and least hurtful, the proper time must be selected; namely, the time when the heart regains its strength.

5. The best guide is the circulation. The sharpness and hardness of the pulse and heart, as felt by you, are a faint picture of the sharpness and hardness of the pulse, as felt by the patient's brain. With your fingers on the wrist, let your mind travel into the interior of the skull.—*British Medical Journal, June* 8, 1861, *p.* 599.

DISEASES OF THE ORGANS OF RESPIRATION.

19.—ON THE UNCERTAINTY OF THE SPUTUM AS A DIAGNOSTIC FEATURE IN PNEUMONIA.

By Dr. Thomas Williams, F.R.S., Physician to the Swansea Infirmary.

The following cases are highly instructive. They convey, in a practical sense, a lesson of great value. It constantly occurs that a case of true pneumonia passes through all its stages to recovery without once exciting the suspicion of the practitioner as to its real nature. It is allowed to pass under the vague name of "influenza," "feverishness," "cold," or "slight fever," &c. There is no expectoration; there is no cough; there is no pain in the side; no obvious symptom is present by which the attention is invited to the chest. If a stethoscopic examination is made, it is done hurriedly and carelessly, and the case is dismissed as something which it is not. These oversights are committed because students are taught by nearly all standard authorities to lay faith in the most variable rather than the least variable symptoms, as signs of the disease.

Another class of cases mislead by the hæmoptysic character presented by the sputum. Pure blood, of a bright scarlet colour, in portions of variable amount, is mixed up with the sputum, the latter being colourless, as in bronchitis. Able and cautious practitioners have condemned such cases as phthisical, and therefore fatal, in which the most complete and lasting recovery has afterwards taken place. Fallacy in this direction is painful in the extreme. The verdict of death is pronounced over those whose life is in no danger. Character and confidence are destroyed, and the medical attendant becomes the greatest sufferer.

In the description of the following cases the strictest brevity will be observed. The fact that a pneumonic consolidation of the lung existed has been carefully determined in each instance by auscultatory examination. To mention the familiar signs of such consolidation were to occupy space unnecessarily; the object being to show how extremely uncertain, variable, and untrustworthy the sputum—the expectoration—is as a symptom of pneumonia.

Case 1.—T. M., aged thirty-eight; a fine, healthy, tall, muscular man. Consolidation of middle and inferior lobes of right lung. Expectoration considerable in quantity for six weeks, blended twice or thrice a day with teaspoonfuls and tablespoonfuls of bright scarlet blood; rest of sputum not rusty nor viscid, but muco-purulent. Was very ill for two months; returned to his employment in three months. Three years have elapsed since the date of this attack; the man is now quite well; the lungs are perfectly free from any evidence of tubercle. The wife and friends have never forgiven me for having said at the time of the attack that he was consumptive and would not recover.

Case 2.—Mr. S., aged forty, a teetotaller, a broad-chested, firmly-built man, a baker and grocer. Nearly the whole of the right lung hepatized. For three weeks was very ill, without any cough or expectoration whatever. Suddenly a cough occurred. "Spitting" of frothy mucus followed. In two days the expectoration amounted to a pint; in four days, to a quart in quantity. It now became most offensive and disgusting to the smell. For a month it continued at this rate, and of this quality. It gradually ceased. In three months the man was quite recovered. In this case the first two stages of engorgement and red hepatization were marked by no expectoration whatever. The third stage, that of grey hepatization, was strikingly indicated by the characteristic sputa.

Case 3.—J. W., aged eighteen. Fine crepitation; dull percussion-note; bronchophony under left scapula; rusty expectoration copious. Was quite well in nine days.

Case 4.—Mr. J., aged forty-six. Pneumonic signs very clear under and around the left scapula; sharp heat of surface; hurried breathing. Was ill for fourteen days. Not the slightest particle of anything coughed up from first to last. No dry cough.

Case 5.—The Rev. Mr. ——'s son, aged seventeen. Very distinct pneumonic signs over a small space of the right lung between the two scapulæ. Was ill twelve days. Threw up once or twice only by cough a slight quantity of clear white mucus. No other expectoration whatever.

Case 6.—W. F., aged thirty. Pneumonia of middle and back portion of the inferior lobes of the right lung. Expectorated large quantities of characteristic rusty sputa. Quite well in sixteen days.

Case 7.—C. M., aged twenty-five. Pneumonia of middle and lower parts of the left lung. Signs unquestionably distinct. Was ill for about a fortnight. No expectoration whatever during any stage.

Case 8.—D. J., aged twenty. Fine crepitation; tubular breathing; bronchophony around the angle of the right scapula. Recovered in seventeen days. Very slight clear mucous expectoration during a day or two.

Case 9.—T. P., aged thirty-five. Pneumonia of the right lung. Signs audible round to the front under the right breast. Was ill for three weeks. Nothing that could be called expectoration at any time.

Case 10.—T. W., aged twelve. Consolidation of the back and side of the right lung; slight pain in side; slight dry cough; no expectoration whatever. Was quite well in thirteen days.

Case 11.—Mrs. D. Pneumonia of the left side; signs very clear under the breast and round under the axilla. Had a little cough, by which mucus only was brought up. Saw a slight streak of blood once only. Recovered in seventeen days.

Case 12.—A. B., a strong working man. Pneumonia of right

lung extensive. Rusty sputa thrown up in copious amount for seven days. Recovery complete and rapid. Returned to work in three weeks.

Case 13.—Master D., a boy in school, aged fifteen. Signs of pneumonic consolidation very clear round right scapula; no pain; no expectoration whatever. Quite well in fifteen days.

Case 14.—Capt. F., aged fifty-five, a very stout man. Attended by the late Dr. Rowland and myself. Fine crepitation of bases of both lungs. Percussion-note dull. Was ill a fortnight, when he died in a typhoid condition. Never had the slightest expectoration.

Case 15.—A. F., aged fourteen, a boy in school. Slight pneumonia of right lung; had no cough; spat up nothing whatever. Recovered in ten days.

Case 16.—D. T., aged twenty-five. Pneumonia of right lung, signs distinctly audible between the two scapulæ. Slight clean mucous sputa. Returned to work in three weeks.

Case 17.—The Rev. Mr. ——, aged twenty-eight, a clergyman. Pneumonic signs traceable clearly from base of right lung forward. Expectorated a small quantity of uncoloured mucus. Was quite well in sixteen days.

Case 18.—Mr. L., a grocer, aged thirty-seven. Right lung extensively pneumonic. Was very ill for nine days, without any expectoration whatever. Suddenly, a severe cough occurred, and threw up large quantities of very offensive semi-purulent sputum; this continued for a fortnight. Was not well under three months. Never had any rusty expectoration.

Case 19.—J. W., aged eighteen. Lung on right side, at base and side, solidified. Was ill sixteen days. No expectoration whatever; once only a little "coloured stuff" came up to his mouth by coughing.

Case 20.—T. P., a copperman, aged twenty-seven. Distinct pneumonia of right lung; middle lobe chiefly affected. Was confined to the house nineteen days. One day a small portion of tinged, not rusty, sputum was coughed up; no other expectoration, but a little white mucus.

During the past winter and spring, ten other cases of this character were noted by me. The notes of each in my note-book are short. They are recorded especially for the purpose of tracing the history of expectoration in pneumonia. On every occasion the diagnosis was carefully and correctly made. In two only out of these ten cases did the distinctive rusty sputa occur.

Thus, in thirty cases, twenty-six of which occurred in the same season, and therefore under the same climatal conditions, five only presented the supposed characteristic symptom, the "rusty expectoration." In one it was hæmoptysical; in two only did grey hepatization take place—that is, if the peculiarly offensive, copious sputum is to be regarded as the distinctive sign of that stage. In twenty cases cough and expectoration were, in truth, totally absent at every stage.

In two the sputa were slightly tinged with blood, as is the case in slight hæmoptysis. In one it was copiously intermixed with bright blood. The colourless mucous sputum, sometimes a little frothy, is the product of the bronchial membrane. Ciliary epithelium, with the cilia in motion, may be constantly detected in it. True epithelial cells are not discoverable in that sputum which comes from the pulmonary cells. The cell-elements of the hyaline epithelium which line these cells are lost in the pneumonic exudation. If, as supposed by Dr. Flint, the contents of the pulmonary cells in pneumonia were nothing but blood, such as it exists in the blood-vessels, and the exudation were, in truth, an act of depletion (virtually blood-letting), rather than an act or product of inflammation, and if the increased weight which occurs in the affected lung were the expression simply of the quantity of blood withdrawn from the system, pneumonia, as as a pathological problem, would indeed have received a highly simplified form. To my understanding, such a solution of the problem, simple only in a mechanical sense, is by no means satisfactory. Place true pulmonary apoplexy and unmixed pneumonia in parallel columns, and compare the distinguishing symptoms; is not the difference most striking? There is scarcely a single point of resemblance. The exudative process constituting pneumonia is not exempt from the law which regulates such processes in other tissues and organs. The diphtheric deposit is a substance which possesses very peculiar and distinctive properties. Although it comes directly from the blood, it is very different from any of the constituent parts of this fluid. The arachnoid, the pericardium, pleura, peritoneum, and tunica vaginalis, when the subject of inflammation, throw out severally fluid products of unquestionably distinctive properties. The pulmonary and alimentary tracts of mucous membranes exhibit corresponding differences. The brain, liver, spleen, and kidney parenchymæ afford each a peculiar history under the circumstances of inflammation. The urine in Bright's disease is not self-coagulable. The fibrine is either detained in the blood or modified in quality. Transudation from the blood, whether it occurs in a direct or indirect manner, is, in truth, tantamount to blood-letting. The serum, lacking fibrine and red corpuscles, which is poured out in serous inflammations, may indeed be described by the free-thinker as blood drawn. The purulent fluid in suppuration would form a link in the same chain of argument.

Such reasoning may satisfy the mere machinist; it is very unexplanatory to the intelligent vital pathologist. The act of inflammation, whether marked and accompanied by a small or a large amount of exudation, is a highly complex *vital* process. The composition of the exudated product differs according to the ultimate anatomical structure of the part affected. Mr. Lister has shown that the solids and fluids mutually react upon one another. What occurs in the fluids must affect the solids; a change in the latter will necessarily involve

the former. Dr. Brown-Séquard has greatly aided our inquiries by proving that an irritation of one part of the nervous system is capable of entailing disease upon a very remotely situated structure or organ by an influence transmitted through the nerves. Local disease from this time forth will be investigated under improved knowledge. In no long time the pathologist will be able to explain *why* a local seizure falls upon one organ or part in preference to another. Already it is obvious that humoralism and solidism, as exclusive doctrines, must be banished from the schools. In studying local pathological changes, the whole act of nutrition should be considered. Beginning with the fluids, the inquiry should pass through the solids, to return again to the fluids. The doctrines of Dr. Hughes Bennett and Dr. Beale are incomplete in this respect, that they suppose visible form, or structure, to be indispensable to vitality and nutritive activity. The liquor sanguinis is a vital fluid: it is a fluid, and is vital. If it is not an amorphous, formless fluid, but a mass of germinal molecules, then inevitably it must be admitted that pure water is not a fluid, but a mass of molecules held together by mutual attraction. The idea of fluidity as a condition of matter must therefore be dismissed from the mind. Solidity is the only possible form!

It is true the chemical philosopher would say all matter is *atomic* in constitution, whether gaseous, fluid, or solid. Consenting, the physiologist would remark that the forces by which the imaginary "atoms" of a fluid are related are lower in degree, bordering nearer on inorganic chemistry, than those which unite into a consentient whole the component particles of a formed, constructed solid. In such abstract reasoning, of course, there is no other difference than that of degree between a solid and a fluid and a gas. But whether a fluid be defined as a formless, homogeneous substance, or a mass of invisible imaginary molecules, it must be conceded that the complex and compound fluids of the animal body are *vital*—that is, that they obey those chemical conditions (probably in a less degree than the formed solids), which constitute vitality. If this be admitted, which appears unavoidable, it must follow that the "round" or cycle of the vital processes do not initiate in the molecules which the microscope can demonstrate to the eye, but in *the fluid* in which the *molecules* arise. The act of germination begins, in fact, at that border line which separates inorganic from organic chemistry. It is, therefore, undeniable that a pathological act, such as inflammation, involves the fluids as well as the solid tissue. The fluids alter in composition either primarily or secondarily, and thereupon follows a change in the mutual relations between the solids and fluids. Under this changed relation certain parts of the blood are extravasated to the retention of other parts. Viewed in this light, the differences in pathological products of different structures under inflammation admit of a satisfactory explanation. The product which fills the pulmonary cells in pneumonia is, undoubtedly, peculiar. That excepted which is contained in vessels of the Malpig-

hian corpuscles, the blood does not lie so near the exterior in any other part of the body as in the pulmonary air-cells. The escape of the blood-corpuscles from the vessels into the cells with the inflammatory exudation, is only an accidental result, not a necessary part of the process. If a large vessel ruptures, a visible quantity of unmixed blood is poured out; if several small ones—capillaries only—the rusty tinge is uniformly mixed with the substance of the sputum. If no vessels at all are mechanically ruptured, no colour appears. The sputa are colourless, exhibiting always, under all circumstances, a singular viscidity, such as is not observed in any other pathological product. It is highly gifted with vital properties; its microscopic growths and changes occur rapidly; it is capable of almost immediate resorption. In a few days frequently a considerable consolidation will disappear. If the affected portion of the lung were simply in a state of ecchymosis, the entire history would be wholly different. As stated by Dr. Walshe, the blood is highly hyperinotic; the serum milky and fatty; the sputum overcharged with chloride of sodium; the urine with oxalates and triple phosphates; the respiration increased in frequency; the skin pungently heated. The whole system, in every sense, sympathizes with the local morbid action. If the disease consisted merely of an act of blood-letting into the lung tissue, as supposed by Dr. Flint, such phenomena would not occur because they would be unnecessary. The argument held by Dr. Flint against venesection in this disease cannot, therefore for a moment be admitted. The exudation into the lung tissue in pneumonia is an act of pathological excretion, determined by two conditions—1, an altered composition of the blood; 2, favourable anatomical conditions afforded by the lung structure for secreting the elements of a peculiar proximate principle from the blood. The condition of the general system, coexisting in time with this pathological act of exudation, may be either sthenic ar asthenic, such as never exists under the circumstances of loss of blood.—*Lancet, Sept.* 7, 1861, *p.* 224.

20.—CHLORATE OF POTASH AND SUGAR IN PHTHISIS.

By Dr. James Turle.

[In an American journal lately there was a short notice of the beneficial employment of large doses of chlorate of potash in phthisis, by Dr. Fountain. Dr. Turle corroborates this. He says:]

I have been in the habit, during the last two years, of giving a very strong solution of this salt, almost *ad libitum*, in tubercular affections. The proportions I use are one ounce to three pints of boiling water.

In the early part of this year I mentioned the subject to Dr. Garrod, who expressed an intention of making a trial of this treatment in suitable cases in the wards of University College Hospital. If he

has done so, his results will be of far more value than the more limited scale on which I have tried it.

Since the end of 1859 my practice has been to combine the administration of from one to three or four ounces of the chlorate per week with the use of three or four pounds of sugar during the same period. When the patient has a dislike for sweets, I substitute the sugar of milk, on account of the little taste of this variety.

Of about twenty cases of confirmed phthisis which have been under my care since the beginning of 1860, the only one which terminated fatally was that of a lady who, from first to last, expressed a most unaccountable repugnance to the taste of the drug, though this is, in comparison with other medicines, very slight, from the small amount water will dissolve. I have, however, found the same objection preclude its use in the case of two other female patients. In but one of the favourable cases had the disease reached the third stage; but in that (the daughter of a tradesman in Portland Town) there was a very large vomica in the right lung, and at the time I commenced the chlorate she was so exhausted by hæmoptysis, hectic, and diarrhœa, that none of those who saw her, including myself, supposed she could live above a day or two. The tone of the stomach was rapidly restored, although there had been nothing like appetite for weeks previously, and her recovery of comparatively good health was uninterrupted. Her general health continued excellent six or seven months subsequently, although she was then described by a surgeon at Brighton as having but one lung left. This young person had been formerly an out-patient at University College Hospital under Dr. Hare. The same gentleman was consulted in another of my cases: that of a man in whom there was very extensive solidification of the upper part of both lungs, with hæmoptysis, and all the other symptoms of rapidly-advancing disease, with softening. His father and all his brothers and sisters had died young, of consumption, and Dr. Hare of course corroborated the very grave prognosis I had given. Cod-liver oil was ordered, but could not be borne by the stomach. Within four weeks of being put on the treatment above described, not only had the emaciation, which had been very great, ceased, but he insisted on leaving off part of the allowance of sugar, on the plea that he was getting too fat. He soon resumed his employment, that of a master blacksmith, and has not since been laid up. I mention these two or three cases to show some of my reasons for thinking favourably of the simultaneous exhibition of these two agents. I have a crude theory on the *modus operandi*, the exposition of which I shall defer to some other occasion when less pressed for time. For the present I shall be satisfied if these hurried jottings should influence some who have the opportunity to make the trial on a larger scale.—*Med. Times and Gazette, June* 1, 1861, *p.* 588.

21.—*New Remedy for Whooping-Cough.* By M. Foster, Esq Huntingdon.—[The remedy in question is the common clover hay (*Trifolium in fœno*).]

While other symptoms have to be met with emetics, aperients, salines, and tonics, its effect in alleviating the spasmodic character of the paroxysms have been very remarkable, so that even if after taking it the cough continues, it is no longer so distressing or exhaustive. I was made acquainted with it in an indirect manner; I believe it came originally from an old fisherman at Hastings.

One has naturally great suspicions about a remedy so purely empirical, but my success with it has been so great, as to cause me to recommend it to my fellow practitioners in these troublesome cases. Since using it I have begun to disbelieve in "the natural course" of pertussis. I have generally found that it answers best when it acts slightly on the bowels.

The hay should be sweet and leafy. The formulæ are as follows:—

The Infusion:—

℞. Trifolii in fœno, ℥ij.; aquæ bullient., Oj.; macerate for four hours and then strain. For a child of five years, take a tablespoonful three times a-day.

The Syrup:—

℞. Trifolii in fœno, ℥ijss.; sacch. cand., ℥ij.; aq. bullient., Oj.; macerate the hay in the water for an hour with gentle heat, then boil down to a proper consistence. For a child of five years, take two tea-spoonfuls four times a-day.

Mr. Squire has prepared for me a fluid extract, which may be obtained from him.—*Med. Times and Gazette, May* 25, 1861, *p.* 561.

22.—ON ARSENIC SMOKING IN ASTHMA.

By Dr. Frederic G. Julius, Richmond.

[The patient was a French lady, who has been subject to spasmodic asthma for twenty-five years, for which she has been bled, smoked belladonna and stramonium, and taken every form of medicine, and changed her residence to various places in Europe,—all without the slightest benefit.]

Four years ago, when at Marseilles, Dr. Cauvin read an account to her of the benefit derived by asthmatics in China from smoking arsenic. Her sufferings were so great that, although Dr. Cauvin fully pointed out to her the risk and danger she incurred, she insisted upon trying it.

She commenced by smoking a quarter of a grain of arsenic three or four times daily in a cigarette, and this she continued to do for about fourteen days, with the greatest benefit to her breathing and general health. She has subsequently much increased the dose, and when

she feels an attack of asthma coming on, she does not weigh the arsenic, but takes up what she considers a sufficient dose with a small paper knife. I asked her to-day to give me in a piece of paper the dose she intended smoking, which she did, and on weighing it carefully I found it a little over three grains. I analyzed it, and found it to be pure arsenious acid. I must also mention the important fact that she does not inhale the fumes and blow them out again as in ordinary smoking, but when her mouth is full she swallows the smoke.

The only ill effects she has ever experienced is swelling of the eyelids, and, when she first commenced, slight pricking pains in the stomach, but never to any troublesome extent. She considers herself cured. From being in a state of constant breathlessness and suffering, unable to lie down or make the slightest exertion, she is now able to go about like other persons, and is rarely threatened with an attack oftener than once in three or four months, and that is at once checked by smoking arsenic with a very small quantity of belladonna or stramonium in the dose I have mentioned. She now uses, instead of a cigarette, a small red pipe about five inches long.

She tells me that Dr. Cauvin has used arsenic in the same way in many cases of confirmed consumption, and has rarely failed in giving great relief and retarding the disease.—*Lancet, Aug.* 10, 1861, *p.* 138.

23.—DRILL FOR AUSCULTATION.

By Dr. Thomas K. Chambers, Fellow and Censor of the College of Physicians, Lecturer on Medicine at St. Mary's School, and Physician to the Hospital.

I place before you a table where you will find seventeen indications which (as a rule) the combinations of the easily recognised signs of pulmonary auscultation afford. It will be very useful to copy them out several times, that you may have impressed on your minds the exact amount of information which the process is capable of giving. Expect this much and no more, or you will be disappointed. Remember that auscultation tells you nothing of the *nature* of the substance whose morbid presence it has enabled you to detect. The "*fluid in the pleura with solid lung*," which "*bulged ribs*," combined with "*dulness*" and "*tubular breathing*," indicate, may be pus, serum, or blood—the history of the case alone can lead you to know which. The "*cavities*" in solid lung may be tubercular vomicæ, pneumonic abscesses, or dilated bronchi—the ear, unassisted by other records, does not enable you to diagnose. Fine or coarse crackling very often cannot be absolutely determined to be inside or outside the lung,—to be the crepitation of the tubes or the rubbing of the pleural surfaces. Do not attempt to decide by auscultation, but by other symptoms.

There are however, a few by-aids to the diagnosis by auscultation which are worthy of seperate mention, in order that you may know

	1. Flat ribs.	2. Bulged ribs.	3. Resonance.	4. Dulness.	5. Tubular breathing or voice.
A. Resonance	Lung recovered from pleurisy, or from condensation.	Air in pleura, or emphysema on contrast with flat side.			
B. Dulness ..	Lung solid and shrunk, from chronic disease.	Fluid in pleura, or aneurism, or malignant tumour.			
C. Tubular breathing or voice	Lung solid and shrunk, from chronic disease.	*If dull*, fluid in pleura, with solid lung.	Tubes stiffened, without solid lung.	Lung solid.	
D. Fine crackling	Recent action added to old disease.	Emphysema and roughened pleura, acute or chronic.	Lobular congestion, early stage of pneumonia.	Lobular congestion, early stage of pneumonia, or pleurisy.	Lung becoming solidified or becoming pervious.
E. Coarse crackling	Lung solid and shrunk, from chronic disease, with muco-pus in bronchi and cavities.	*If resonant*, emphysema and bronchitis; *if dull*, fluid in pleura, with adhesions and bronchitis.	Bronchitis, with slight, if any, consolidation.	Solid lung, with muco-pus in bronchi or cavities.	Much muco-pus in large bronchi or cavities.

how to use them worthily, and to caution you against their abuse.

1. *Whispering Pectoriloquy.*—When the patient whispers a few words of several syllables, and the distinction between the syllables is clearly heard, it is often taken as a sign of a cavity. It is an absolute sign of *diseased lung*, but not an absolute sign of a cavity, unless it is found in a limited spot with a clearly defined line, outside which line the voice is quite different.

While on this subject I will give you a hint about auscultating the voice. Always make the patients speak in words of several syllables: make them count "twenty-one," "twenty-two," &c.; not "one," "two." You get twice as much information thus.

2. *Cracked-pot Sound.*—A jingling noise like a cracked pot, or more like the striking of the clasped palms on the knee, as when we amuse children by imitating the jingling of money, is often produced by percussing a superficial large cavity while the patient holds the mouth

open. But it is not a sign absolute. I used to show you last year in the wards a child with a cavity at one apex, where the cracked-pot sound never occurred, though it was remarkably distinct almost always in a perfectly healthy part of the opposite lung. It is caused exactly in the same way as the imitative money jingle in the closed hands—namely, by the air being *jerked* out of a partially open space. When the parietes of the chest are thin, it may thus be produced by the jerk of the air in the trachea or large bronchi. Do not, therefore, set store by it as a sign.

3. *Metallic Tinkling.*—This is a valuable sign of a very large cavity with small bubbles bursting in it. It is like the sound of a small shot or fragment of gravel falling in a thick metal vase. It shows the existence of either a vast vomica, or of air in the pleura, with a communication open to the bronchi.

I will now go through the eighteen simple combinations into which these tabulated signs may enter, and point out what information you get immediately from them, and in what direction you are to seek for more on which to ground your diagnosis.

Combination A 1.—The *flattened ribs* show that some morbid state has existed; there has been either inflammation of the parietal pleura, which has thickened it with contracted scars, or a condensation of the lung for a sufficiently long period to leave cicatrices, and to diminish the volume of the organ. But the recovered *resonance* shows that the lung is again at work—that the fight is over, and that corn again grows on the battle-field.

A 2.—Be quite certain that the *bulging of the ribs* is not merely apparent, and that the other side, being flattened from disease, does not make them seem too rounded and prominent by contrast. If they are really *bulged*, and yet resonant, there must be a larger body than natural beneath the point of percussion. And this air must be either in the pleura (pneumo-thorax) or in the lung (emphysema). If the former, you find a peculiar resonance like that produced by tapping the stomach under the ribs, you find the breath-sounds quite absent, and probably also "metallic tinkling." If the latter, you have breath sounds, perhaps bronchial breathing, and not unfrequently a peculiar crackling like the crumpling of fine tissue-paper. When you hear this "crumpling," it is very distinctive. It arises, I would suggest, from the rubbing of the rugged, bubbly-looking pleura of the lung against the parietal.

B 1, C 1, and C 4.—Solidification of the lung makes it dull, and makes the voice and breathing bronchial or tubular, whether arising from pneumonic acute condensation, or from such a chronic cause as tubercle. But if the cause be chronic, the ribs will be flattened in proportion to its chronicity and the length of time it has endured.

B 2.—*Dulness* on the *bulged part* of the ribs shows that some matter more solid than air is bulging them. In the lower part of the chest it is most probably fluid, and then you must try, if you can,

a confirmation of your diagnosis by making it move about as the patient changes his position from one side to another. In the upper part it may be an aneurism of a trunk vessel, and then you get pain on pressure of the corroded ribs in the place where they are gnawn away by the lump inside. To the diagnosis of a pectoral malignant tumour you can arrive only *par la voie d'exclusion;* but in all the cases I have seen, the veins, by their remarkable prominence in the immediate neighbourhood, have led me to the suspicion.

C 2.—The only case where bulging of the ribs is likely to be joined with tubular breathing is where a lung adherent to the ribs is condensed by the pressure of fluid, and then absolute dulness is of course produced.

C 3.—The larger air-tubes sometimes become stiff and hard from chronic degeneration without any hardening of the lung-tissue, especially in old people, and then you get *resonance* combined with *tubular sounds.* The knowledge of this chronic degeneration is important, as it leads you to expect future degeneration in more important viscera, —such as of the lung-tissue, in the form of emphysema; of the kidneys, as Bright's disease; of the heart, as dilatation, &c.

D 1 and E 1.—The flat ribs show that there is chronic disease of some standing, and this will modify the activity of your treatment of the acute action indicated in its first stage by the fine crackling, and and in a later stage by the coarse crackling. In such a case as this, the fine crackling is an evidence *against* a vomica; while the coarse crackling (especially if bounded by a sharply-defined line) is the best single evidence you can have of one being already formed.

D 2.—The condition of fine crackling or crumpling as a sign of emphysema has been just mentioned under B 2.

D 3, D 4, and D 5.—When only separate lobules are congested (as in the first stage of pneumonia), the quantity of air in the lung is not sufficiently diminished for dulness on percussion to be distinguishable by the ear; but when the whole substance of the lung is infiltrated with blood or serum or fibrin, instead of air (as in a more advanced stage), comparative dulness is to be perceived. However, this is not such absolute dulness as in a further stage, when it is so solidified that no air enters. Then fine crackling ceases, and silence or coarse crackling or tubular sounds take its place. Then, as the lung again becomes pervious in the course of cure, fine crackling returns; the acoustic condition of *becoming* solid and of *becoming* pervious being the same, though the fluid which causes that condition is different.

E 1, 2, 3, 4, 5.—A *coarse crackling* is distinctive of nothing but the immediate cause of the sound—viz., a thickish fluid in the area where it is found, whether bronchi or vomicæ. It may be associated with all sorts of conditions of lung. You hear sometimes physicians pronounce that there is "bronchitis" in a certain case, and expect those who have consulted them to be satisfied with such a diagnosis,

and nothing more. It is no diagnosis at all, and certainly not worth a guinea; for it helps in no degree in the treatment, and gives no information available for prognosis. Every patient with emphysema, chronic or acute catarrh, hard tubercle, vomicæ, pleurisy, and what not, has from time to time a secretion in the bronchi (or bronchitis), and to declare with grave sapience that "there is bronchitis," is simply to put into bad Latin what the patient has been telling *you*—namely, that he has got a cough. No; when you do take your patients to a physician, get something more out of him than that.—*Lancet, Oct.* 19, 1861, *p.* 369.

DISEASES OF THE ORGANS OF DIGESTION.

24.—CURIOUS CASES OF FURRED TONGUE ON ONE SIDE, FROM VARIOUS CAUSES.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital, &c.

Furred Tongue on one side caused by a Decayed Tooth on the same side.—As I purpose making the case of gangrene of the fingers from pressure upon the ulnar nerve the culminating point of a little group of cases bearing upon the subject of physiological rest, or rather the evil resulting from the want of physiological rest to the nerves, I must request your attention to the case of a tongue furred on one side, and comparatively clean on the other. Now, this furred condition of the tongue, let me say in anticipation of what I have to speak of more in detail, is frequently associated with a diseased condition of the second division of the fifth nerve; in fact, it is a functional and structural deterioration depending upon nervous influence, in that respect very much resembling what we have already seen in its extreme condition in the case of gangrene of the fingers.

Furred Tongue on one side, depending on Disease within the Cranium.—The first time I had an opportunity of clearing up such a case as this occurred in 1843. On December 17th, 1843, with Mr. Blenkarne, a surgeon in the city, I examined the body of one of his patients, who had died with disease of the brain and spine. She had suffered from intense pain on the left side of the head. She had also during her life, a furred tongue on the left side, and scarcely at all on the right side. It therefore became an important point to clear up what was the probable cause of that condition of the tongue. We made a post-mortem examination, and found a diseased spine, as was anticipated. Then, upon very careful examination of the head, for the especial purpose, on turning up the dura mater from the anterior part of the petrous portion of the left temporal bone, we exposed the

Gasserion ganglion, or the ganglion of the fifth nerve, and there we found what might be called a scrofulous deposit upon the convex edge of this ganglion, involving the second division of the fifth nerve, more than the third, but still involving all more or less. Here, then, appeared to us to be the explanation of the probable cause of the furred tongue on its left side. This observation was made in 1843; the disease was near the ganglion of the fifth nerve, and on the same side as the furred tongue. If I should not mention that fact again, it will be clearly understood that the diseased condition of the tooth, or of the fifth nerve, was always found on the same side as the furred tongue, and that the fur was confined to the anterior two-thirds of the upper surface of the tongue, over the distribution of the lingual gustatory nerve—a portion of the third division of the fifth.

In 1844, when delivering some lectures upon the nerves at Guy's Hospital, I mentioned this case, and one other that I had seen before, the cause of which I had not distinctly proved. After the lecture, one of the students said, "Look at my tongue; it is furred on one side, and I cannot get rid of it." He had a decayed and painful tooth—the second molar in the upper jaw. That tooth was, by my advice, removed, and after a short time the fur on the tongue entirely ceased. Now, here, in this case, appeared to be a demonstration, as far as we could judge from these concurrent circumstances, of the probable cause and effect. Here was a painful and diseased second molar tooth in the upper jaw, on the same side as that on which the tongue was furred, and apparently the cause of that condition; for as soon as the tooth was removed the fur on the tongue ceased, and the two sides became quickly alike. This case seems to confirm the opinion as to the cause of the unilateral furred tongue; but there is this peculiarity and difficulty in the explanation, which I am anxious to admit,—that instead of the cause being associated with the third division of the fifth, as you might expect from the lingual gustatory nerve supplying that part of the tongue which is furred, in all the cases which I have seen where the cause was connected with the teeth the seat has appeared to be the first or second molar tooth of the upper jaw (the second division of the fifth nerve) on the same side as that on which the tongue is furred. I must leave others to explain how that occurs. I am responsible for the accuracy of the facts which I have advanced, but I cannot explain the relation of cause and effect in these instances.

Case of Fractured Base of the Skull, with Furred Tongue on one side only.—Some time afterwards a case of fracture of the skull came into Guy's Hospital, where the fracture ran across the foramen rotundum containing the second division of the fifth. In that case the man had furred tongue on the same side, indicating the probable relation of cause and effect.

When Mr. Bransby Cooper was alive, a gentleman was brought to Guy's Hospital, who was believed to have fractured the base of the

skull, and being anxious about the case, he requested Sir Benjamin Brodie to see him. That patient had a furred tongue on one side, and he had also indications, from the loss of sensation about the face, that the line of fracture interfered with the second division of the fifth. He had a furred tongue on the same side as that which we supposed to be the seat of injury during the whole time he was in the hospital.

Furred Tongue on one side; Second Molar Tooth of Upper Jaw Diseased.—This last spring, a lady, whom I have known for some years, consulted me regarding some matters not of importance, and I, perhaps not for any very precise purpose, said, "Let me look at your tongue." She put out her tongue, and it was furred on one side. I said, "You have a bad tooth," and she thought it was exceedingly clever on my part. "Yes," she replied, "and I am going to Mr. Bell to have it taken out" She went and had it extracted; I saw her a fortnight afterwards, and all the fur had subsided. This was a second molar tooth in the upper jaw of the same side as the furred tongue.

Very recently my own boot-maker had some little accident, and he came to my house to consult me. I found he had a furred tongue on the right side, and I remarked to him, "You have a decayed tooth in your upper jaw." "No," he said, "I have not; but I have had a bad tooth stopped, and it is very painful when I touch it in this way, (pressing his finger upon his cheek over the tooth;) almost the whole of the time since I had my tooth stopped, my tongue has been like this."

Now these cases, taken together, and made into a series or small group, seem strongly to suggest the influence of irritation or of pressure upon what we may call common spinal nerves grouping the fifth nerve as a common spinal nerve, although it has its relation only with the face and the head.

Grey Hair on the Temple depending on a Decayed Molar Tooth in the Lower Jaw on the same side.—This spring a person was brought to me by a surgeon suffering very great pain on the left side of his face. He was much exposed to the weather, and he suffered a great deal in consequence. He had taken many things to cure the neuralgia, as it was termed. I observed that he wore a wig, and I asked him the reason. He said, "Curiously enough, the hair on my left temple has all turned grey. I did not like to have black hair on one side and grey on the other, so I had my head shaved and wear a wig." Upon examining his mouth I found he had a decayed and painful molar tooth on the left side of the lower jaw—the third division of the fifth nerve. The patch of grey hair appeared to me to be the effect of the nervous association of the temporal branches of the third division of the fifth nerve and the decayed tooth in the lower jaw. When this second molar tooth in the lower jaw was extracted the neuralgic pain very nearly ceased. I have not seen the patient since, and cannot say whether the hair has recovered its colour. All

I can say is, as it was stated to me, that during the time he was suffering extreme pain on the left side, the hair over the temporal region became nearly white—that difference in colour suggesting to myself some structural deterioration, and to the patient the propriety of having his head shaved and wearing a wig.

Diseased Molar Tooth of the Lower Jaw producing Excoriation of the Auditory Canal on the same side, and an enlarged Lymphatic Gland, cured by removal of the Tooth.—In last year's lectures I mentioned that a professional friend of mine had suffered from a condition bearing on the subject of the influence of nerves upon the structures supplied by them. That friend, unfortunately, is no more. It was Dr. Addison to whom this happened. The case is one of some interest, and I will repeat it in a few words. Some years ago Dr. Addison had a very offensive discharge from the auditory canal of one of his ears, which annoyed him very much; and below the external ear was a small gland enlarged in the upper part of the neck. He had tried various remedies for this discharge, and had gone, I believe, to some surgeons who attended specially to the ear; but, as far as I could learn, no good resulted from any of the applications. Upon examining the ear from which the offensive discharge proceeded, I found a slight ulceration upon the floor of the auditory canal. On arguing the question out between us, we came to the conclusion that the ulceration probably depended upon a diseased molar tooth in the lower jaw on the same side. We had that tooth extracted, and in a very short time the ulcer healed, the discharge and morbid secretion disappeared from the auditory canal, and as soon as that ulceration was cured the enlarged gland subsided. Here the true cause of error was remotely situated from one of the most prominent symptoms,—namely, the enlarged gland in the upper part of the neck: in that respect the case is of great interest. The case stands also in nice and close association with this group of results of nervous disturbance which prove that irritation induced in a nerve, or pressure upon a nerve, is sufficient to lead to more or less change in function and structure, and that that morbid influence may after a time induce a deterioration resulting in, or sufficient to produce, a furred tongue, ulceration of skin, or gangrene of the fingers. In Dr. Addison's case there was an ulceration in the auditory canal, and the gland enlarged as the consequence of the exposed lymphatics absorbing and carrying the morbid fluid of the ulcer down to the lymyhatic glands. That I believe to be the true interpetration of glandular enlargements from the irritation, as it is termed, of a remote cause; the "irritation" is supposed to travel along the lymphatics to the anatomically associated glands. I believe this hypothesis of irritation is an erroneous idea, and that the true explanation is, the arrival at the gland of a morbid fluid conveyed to it by the lymphatics. I doubt the validity of the reasoning which endeavours to press upon us the conviction that mere irritation can be conveyed from a sore to a lymphatic gland; it must

be that the absorbents carry a morbid fluid centripetally to the gland. Fortunately for us, these glands are highly conservative in their tendency, and they stop the progress of the morbid fluid in its onward course towards the circulation of the blood, where its damaging influence might be more diffused, and lead very frequently to the occurrence of the more serious general condition which we are in the habit of denominating pyæmia.—*Lancet, August* 31, 1861, *p.* 201.

25.—ON DIPHTHERIA.

By Dr. William Budd, Clifton, Senior Physician to the Bristol Royal Infirmary.

[It must be well known to our readers what a great variety of remedies have been suggested in this disease, each having its warm supporters.]

About two years ago, on the strong recommendation of my brother, Dr. G. Budd, I began to employ Beaufoy's solution of chlorinated soda, undiluted. Before that time, I had tried most of the preparations in vogue; but this one has seemed to me so superior to them all, that I have since abandoned every other in its favour. In my hands, it has appeared to have more than all the advantages of the other remedies in common use, without any of their drawbacks.

In all the cases in which I have hitherto tried it—amounting now to fifteen—the local disease has begun to decline from the moment of its first application. An immediate subsidence of swelling in the lymphatics in the neighbourhood of this disease, has, in particular, always followed its use—a fact from which the inference seems pretty clear, that this agent may do much to prevent that secondary poisoning of the system by the absorption of putrescent matters from the throat, which, in many cases, has the chief hand in giving a malignant turn to the malady.

On the other hand, its employment is attended by little or no pain; it causes no inflammation, and it leads to none of those breaches of surface which are inseparable from the repeated use of powerful escharotics, and which, when once produced, often make convalescence so painful and protracted.

Lastly—and this alone is a consideration of the highest importance—by exercising a local disinfection in the very focus of the diphtheritic virus, there is reason to suppose that this preparation greatly lessens the contagious power of the malady.

A full-bodied camel's-hair brush is the best instrument for applying it; and to insure its full efficacy, both as a curative and a sanitary agent, the throat should be thoroughly mopped with it as many as three or four times a day.

The general treatment adopted was, in all cases, in entire accordance with that now almost universally followed, in so far as this

consists in a supporting plan, and a pretty liberal use of alcoholic liquors.

Diphtheria is the effect of a poison, whose chief characteristic it is to produce rapid septic changes; and to enable the body to resist these changes, an abundance of good nutriment, and a sufficiency of wine or spirit, are indispensable.

These are matters which, in this country at least, are so well understood, that I need not enter further into them. The only speciality to which I have to refer in connection with the medical treatment of the cases which form the subject of these remarks, was the administration of the tincture of susquichloride of iron in considerable doses. *British Med. Journal, June* 1, 1861, *p.* 575.

26.—ON THE TREATMENT OF ACUTE DYSENTERY BY IPECACUANHA.

By Dr. R. W. Cunningham, Assistant Surgeon to H.M. 4th Bengal Europeans.

[It is only lately that ipecacuanha has been used in large doses in tropical dysentery, though it has long been known and used as one of the best remedies in that disease. It was formerly given in comparatively small doses; now in doses of ʒi.—ʒiss.]

On admission, a sinapism is applied to the epigastrium, and ℥xxx. of tincture of opium are given, for the purpose of rendering the stomach more tolerant of the remedy. An hour afterwards, one drachm to one drachm and a half, of ipecacuan is given in powder. Mr. Docker recommends it to be given half an hour after the tr. opii.; but, by waiting for a whole hour, the medicine is retained longer, and produces a more powerful effect. As may be supposed, considerable nausea is the result; but vomiting does not generally come on until at least an hour afterwards, sometimes two hours. During this nausea, copious perspiration is produced over the whole body; the pulse becomes more full and soft, and less frequent; the countenance loses the expression indicative of suffering, so marked in acute dysentery; tenesmus and abdominal pains immediately cease, and the patient has no more stools for 12, and sometimes for 24, hours. As soon as the nausea passes off, he expresses himself immeasurably relieved. The perspiration is kept up for a considerable time, and, of course, he feels languid and depressed for a little; but this has not, in any case that I have seen, proceeded so far as to call for any interference. When the patient next goes to stool, he does so without pain or tenesmus; the evacuations are fluid, and hold in suspension small masses of fæces, but are entirely free from blood or mucus. Sometimes the first dose is not sufficient, the dysenteric stools returning after a short interval; in which case a repetition of the dose will be required. Chicken broth and arrow-

root is the only diet allowed. In most of the cases that I have met with nothing further has been required, after the action of the ipecacuanha, than to keep the patient upon the above-mentioned diet for a few days, until the bowels recover their tone; or, if any tendency to debility manifested itself, a few doses of infusion of chiretta with nitro-muriatic acid have been sufficient to complete the cure.

The *rationale* of the action of ipecacuanha, thus administered, appears to me to be, that it produces the same relaxing effect upon the mucous membrane of the bowel that it does upon the system in general: a copious flow of the secretions of that membrane is produced, and an effect similar to that which we produce when we scarify an erysipelatous surface is the result.—the membrane is relieved from the tension caused by the congestion and effusion of lymph, thereby preventing destruction of tissue and ulceration. The excreted fluids lubricate any scybalæ that may be retained in the saccules of the colon; and the general nausea overcomes the spasmodic action of the muscular coat, thereby enabling the scybalæ to be set free. But the action of the ipecacuan does not end here; enough is retained to produce a cathartic effect; or it may be that the whole mucous surface of the bowels has been induced to pour forth an increased secretion during the nausea; at all events the stools become copious, fluid, and fæculent. This catharsis continues for several days, thereby relieving any abdominal congestion that may exist, by causing a drain from the whole portal system; and it possesses this advantage over that produced by most purgatives, that it is mild, free from irritation, and neither constipates nor debilitates the bowel after it has ceased.

Of the cases treated by me, or which I have had an opportunity of seeing, not one has had a relapse after leaving hospital; and, on looking to the old Registers, I find that cases similar to those I have seen were, under other systems of treatment, much longer in hospital, and also very liable to relapse. Army surgeons have this advantage, that all acute cases of illness must come under their observation in the very commencement; so that, in most cases of acute dysentery, the patient is under treatment while the disease is yet in its first stage, and no actual destruction of tissue has taken place. The means is therefore invaluable by which we can arrest this frightful disease in its infancy, before it has advanced to the stage of ulceration, rendering necessary a long and tedious course of treatment, and too often giving rise to serious complications in other organs than the one originally affected in simple acute dysentery. By overcoming the disease at the onset, we at once relieve the general abdominal plethora with which it is associated; and thus, though the small intestine, mesenteric glands, liver, and spleen, be at the same time in a state of active congestion, the action of ipecacuanha as above described, is not only efficient in removing the actual inflammation of the organ primarily affected, but is even more so in overcoming the congestion of

other organs, so intimately related as these are with the principal seat of the disease.

Though my experience of this disease has been very limited, I think we may fairly assume, on these grounds, that the occurrence of hepatic and other complications will be far less frequent, and chronic dysentery much more rare, than under other systems of treatment which require a longer continuance of the remedies, and thus allow active congestion of important organs to ripen into actual inflammation.

I append the following case as illustrative of the previous remarks. It was acute and uncomplicated. The patient was six days in hospital; but, after two days, may be said to have been convalescent, as the only treatment consisted in a regulated diet, no medicine whatever having been given.

Pte. J. L., aged 28, a stout, healthy-looking man, was admitted on the 20th October, suffering from acute dysentery. Has been to stool twice at hospital within a few minutes, and says, that during the whole night he has been to stool every half hour. He has much tenesmus, intense heat and pain in the rectum, deep-seated pain and burning sensation in the left iliac region; skin hot and dry; pulse small and frequent, about 100; tongue slightly coated. The stools contain no fæculent matter, but consist entirely of mucus, with bright fluid blood.

℞. Tr. opii ℥xxx. stat. sumend.

℞. Emplast. sinapi epigastrio applicand. et post horam.

℞. Pulv. rad. ipecacuanhæ. ʒiss.

Diet.—Chicken broth, with ℥i. of arrowroot added to one pint.

Vesp. The medicine was retained for a very short time. There is a little improvement in the stools, but they are still frequent, and attended by much tenesmus.

Repeat.—Medicamenta supra scripta.

21st. Has had no stool since he took the ipecacuan last night. Retained the dose for two hours. All abdominal pain and tenesmus have entirely passed off. Deep pressure in the left iliac region does not now give pain. Skin cool and moist; pulse more full and soft, 90.

Vesp. Has had no stool; says he is quite well.

22nd. Has had a small stool this morning, fluid and fæculent, without any dysenteric appearances, and without pain or tenesmus.

Vesp. Has had a large fæculent stool, fluid.

23rd. One stool this morning; becoming more solid. Continues quite well, and complains of not having enough to eat.

24th. Continues well; has had no stool since yesterday. To have mutton chop ℥viii. in addition to the former diet.

Discharged on the 26th October, quite well; and now continues so on the 1st November.

This case affords a good example of the success of the ipecacuanha major plan of treatment. The failure of the first dose I attribute to its not having been retained sufficiently long, the nausea and other effects were consequently very transient, and insufficient to produce the desired result.—*Edinburgh Med. Journal, July* 1861, *p.* 26.

27.—ON THE TREATMENT OF DIARRHŒA BY NITRIC ACID AND OPIUM.

By Dr. Patrick J. Hynes, Nottingham.

[Dr. Hynes recommends an old favourite remedy, which is sometimes forgotten, and we therefore think it well to let the reader see it again. He says:]

About ten years since, diarrhœa and dysentery prevailed rather extensively in this locality, and emboldened by the successful treatment of these diseases from the remedy I was in the habit of employing, I ventured to bring it, through the medium of the public journals, into notice, and since that time I have been repeatedly assured by many gentlemen who adopted it, that my opinion of its efficacy has been by no means exaggerated. I believe I am indebted to the work of Dr. Abercrombie 'On Diseases of the Abdominal Viscera,' for the formula I am in the habit of employing; but as I write from memory, and have not an opportunity of confirming my opinion on this point by a direct reference to that excellent work, I do not wish to deprive any other author of whatever merit may be found to attach to its introduction as a remedy for those diseases.

I may observe that during the last four or five weeks diarrhœa has been very prevalent in this locality, and I do not overstate when I assert that I have used it in more than a hundred cases already, and always with almost instantaneous benefit. The formula that I generally employ is as follows:—Compound infusion of gentian, eight ounces; tincture of opium, a drachm to a drachm and a half; nitric acid, twenty minims: one ounce to be taken after every liquid stool or painful alvine evacuation. A mustard plaster applied to the epigastrium, and drinking sparingly of ice-cold mint-tea, relieve the sickness and thirst that frequently accompany the severer form of these diseases. I have found the remedy equally beneficial in some rather intractable forms of dysentery. I would strongly urge upon gentlemen, who meet with dysenteric cases, to give it a trial, as I feel satisfied they will find it a very powerful curative agent in their hands.

In an old number of the 'Medical and Physical Journal,' from which several years since I made an extract, I find the following from a Mr. Hope, of Chatham. He says:—

"The first occasion of its use was remarkable. A young man, of sobriety and temperance, had suffered long with dysentery, and had

been attended by a friend of mine for some time, who recommended those remedies that high authority and experience pointed out for relief; but finding no advantage, he sent for me, expecting more might be done. Nothing, however, recommended was successful, and, as I could but go over the same ground, no prospect of relief appeared; indeed his death was daily expected. At this time a woman who lived with him was attacked with dysentery, with extreme thirst. An acid occurred to me; but. fearing it might produce unpleasant effects, opium was added:—Nitric acid, two drachms; opium, two grains; water, two ounces: a spoonful to be taken in any vehicle three or four times daily. The effect produced was so great that my dying patient, unknown to me, begged to partake with her, and when I saw him next morning, which to my great surprise was with a cheerful countenance, he told me if ever I had a patient ill with his complaint, I should never fail to send the drops I had sent for the woman. for they had relieved his complaint at the first dose, and he was sure he should mend now, for they had saved his life this time. This was the only medicine he took, and in a few days he was able to walk about his room. In a third case I tried the acid without the opium, but it did not succeed. I then united them, and it effected immediate relief. I was still unwilling to persuade myself into the belief of its being a specific remedy, until a case of so extraordinary a nature occurred as compelled me to decide unequivocally in its favour. A young lad, sixteen years of age, fell over a dredging-boat into the water in the month of July last. Indisposition succeeded, but to what degree I could not determine. as another practitioner was at first called in to his assistance. Finding no relief, they applied to me, requesting my attendance. This was near a month after the accident. The remedies I applied failed in their efficacy. His friends, despairing of relief, requested me not to trouble myself to attend him any more (they lived six miles from me), saying he must be left to his fate, being assured a day or two more would finish the scene. At this time I recommended the anodyne, and with great difficulty it was that they could be persuaded to give it a trial. Twenty-four hours had not elapsed before he began to find relief, so that in four hours he left off the remedy. The disease returned; reapplication of the drops again removed it, and in a very short space of time, without any other medicine intervening, he became as hearty as ever he had been. The manifest advantage of the medicine was recognised by the parents; nor did they spare unjust reflections that I did not employ it before."

I am unwilling to theorize upon the *methodus medendi* of what I believe to be the principal agent in the above formula; yet I cannot help thinking that the nitric acid possesses some disinfecting agency, no less than an astringent efficacy, over autumnal diseases. "The fumes of nitric acid are believed to be efficacious," says the late Dr. Montgomery, "in destroying the effluvia of typhus and other febrile diseases." Diluted with water so as to form an acidulous drink, Dr.

Duncan of Edinburgh used to employ it in the low fevers that prevailed in the suburbs of that town. But independent of its chemical action over animal effluvia, it appears to me to act as a direct astringent in all diseases of the mucous membrane. Thus in purulent ophthalmia, what remedy is so efficacious as nitrate of silver in solution? In fact, in all mucous discharges it is almost the sole remedy upon which the surgeon depends; and of course the nitric acid is the chief agent in this valuable therapeutic. I am in the habit of employing a formula of a nearly similar kind as a topical application, but with double the amount of acid, in cynanche and in diphtheria; and I can speak very strongly upon its beneficial effects. In broken-down constitutions impaired by mercury, by syphilis or other irregularities, the above remedy will be found frequently valuable; and given in combination with taraxacum, it will prove very serviceable in sluggish conditions of the liver. I am in the habit of employing it with very considerable advantage in the diarrhœa of infants, and, combined with the muriated tincture of iron, in tabes mesenterica. I may further state that I have given the other acids, singly and in combination, the full benefit of a fair trial in all the above forms of disease; and, without any prepossession or prejudice, my experience enables me to give a decided preference to the claims of the nitric acid in combination with opium, as a very superior therapeutic remedy. I have not had an opportunity of making trial of it in the very severe forms of epidemic cholera, although I have had formerly ample opportunities of treating that disease. I have, however, many years ago, tried extensively, *inter alia*, the plan of treatment recommended by Dr. Toulmin of Brighton, thus anticipating his views (see the 'Lancet' of August 17th), which certainly look well in print, particularly as they are eloquently, indeed elegantly, introduced; but when weighed in the balances they will be found wanting.—*Lancet, August* 31, 1861, *p.* 204.

DISEASES OF THE URINARY ORGANS.

28.—ON THE DISEASES OF THE KIDNEY, ACCOMPANIED BY ALBUMINURIA.

By Dr. C. J. B. Williams.

[The following remarks were drawn forth by a paper read before the Royal Medical and Chirurgical Society by Dr. Dickinson. Dr. Williams commenced by suggesting the question as to whether the granular and smooth mottled forms of kidney disease were not different stages of the same affection.]

Many years ago, when more particularly investigating this subject, he had arrived at the opinion that the smooth and mottled kidney was the early form of the disease, which at first was attended with

smoky urine from the addition of blood, and that, subsequently, as the kidney advanced to a granular condition, there was no blood and less albumen in the urine, which was of a low specific gravity, for want of its natural constituents. He believed that he had traced some of those cases to an original congestion or inflammation of the kidney, as, for instance, after scarlet fever. Such cases would be better for treatment, and many were quite cured; but others in subsequent years would become liable to kidney disease, ending ultimately in granular disease. He quite admitted the anatomical differences, of disease of the cortical and of the tubular parts of the kidney, yet he considered that such differences were different stages of one disease rather than separate and definite affections. Cold, or the scarlet fever poison, might attack the tubules, and fill them with diseased epithelium (acute desquamative nephritis), endangering life by their obstruction; but suppose this danger escaped, the same cause might proceed insidiously to attack the cortical structure and produce the granular kidney. He would mention some facts connected with the association of degeneration of the kidneys with organic disease of the heart, cerebral apoplexy, and consumption. The belief in this connexion had been forced on him so strongly that he could have no doubt of it. There may be some doubt whether the affection of the heart or of the kidney took place first. He was quite sure that in some cases of heart disease the kidney became affected, and he believed that often disease of the heart killed by inducing disease of the kidneys. He had shown that disease of the heart *per se* is not necessarily a fatal disease, that the patient may live, provided he lives in a lower grade of vitality, a reptile's life, and so he may exist for many years; but when any great excretory organ became affected, then would come the beginning of the end; and no organ was more likely to be the immediate cause of death than the kidney. Simple hypertrophy would lead to the same results in extreme cases, in which the force of the circulation caused an exudation of serum into the urine. On the other hand, the condition of the blood in renal disease had a peculiar effect on the action of the heart. He had seen cases in which, from a particular sound of the heart, he had been led to suspect disease of the kidney, sometimes a reduplication of the second sound, but more frequently of the first, what is often called the "cantering sound." He had seldom heard this without also finding some renal affection. Purgatives sometimes relieved this condition of the kidneys, and then the cantering sound and signs of hypertrophy would cease. The association of phthisis with renal disease was not so common, as phthisis destroys life before the kidney can become extensively affected. But in instances where by care and treatment phthisis was protracted for very long periods, for years, he had seen death from granular disease of the kidney, as proved by post-mortem examination. He admitted that in a large majority of cases this disease was very intractable, but it was not uncommon to see old, gouty

people, whose urine every now and then contained a little albumen and was of low specific gravity, and yet who lived for a great length of time. It was in his experience rare to find a kidney not somewhat granular after forty, and as such persons generally die of some other disease, it might reasonably be supposed that there were means of at least retarding the progress of the disease during life, just as phthisis might sometimes be kept comparatively quiet for many—even thirty or forty—years.—*Med. Times and Gazette, June* 8, 1861, *p.* 615.

29.—ON URÆMIC INTOXICATION.

By Dr. W. A. Hammond, Professor of Anatomy and Physiology in the University of Maryland.

Professor Hammond has a long article on this subject in the 'Amer. Jour. of Med. Sciences' for Jan. 1861, in which he contraverts the opinion of Frerichs, that uræmia is due to the conversion of urea into carbonate of ammonia, and ascribes the poisoning to direct action of the elements of the urine retained in the blood, upon the brain and nervous system; and among these elements, urea may be deemed the most poisonous. Frerichs performed two series of experiments upon which his theory is based: the first of which consisted in injecting into the veins of dogs, the kidneys of which had been previously removed, a solution containing from thirty-one to forty-six grains of urea. In a period varying from one and a quarter to eight hours, the animals became restless, vomiting and convulsions followed, ammonia was expelled with the expired air, and coma and death supervened; after which ammonia was found in the blood, stomach, bile, and other secretions. In the second series, a solution of carbonate of ammonia was injected into the circulation, the kidneys remaining intact; convulsions occurred immediately, followed by coma, the expired air meanwhile loaded with ammonia. As this ceased to be expired the animals slowly recovered. The author considers this no evidence that carbonate of ammonia was the cause of death, but on the contrary that ammonia may generally be detected in the respiration of healthy dogs, and is generally present in the blood of most animals, the truth of which he has satisfied himself of by repeated observation and experiment.

It is admitted that urea is a normal constituent of the blood in variable proportions; and it is only when the kidneys fail to eliminate in accordance to the requirements of the system, that the normal balance between the urea and blood is disturbed. In support of his views, the author cites three series of experiments, the first of which was the injection of urea into the blood of sound, healthy animals. Large dogs were fed for three days on meat, during which time ammonia was constantly found in the breath, and urea in the blood. Upon injecting a solution of urea, symptoms of uræmia were soon apparent,

of a severity in proportion to the amount injected. Upon a second examination of the blood, the urea had increased in quantity, while the ammonia remained about the same. "From the foregoing experiments it is perceived that there is a limit to the power of the system to eliminate urea, and that when this substance is introduced into the blood in large quantity, it causes death by uræmia."

The second series of experiments had reference to the effects following ligature of the renal vessels, or removal of the kidneys. For this purpose large dogs were selected, urea found in the blood, and ammonia in the breath before the operation. After removing the kidneys, or ligaturing the arteries, the amount of urea was increased threefold in a short time, and there was no evidence of any of it being converted into carbonate of ammonia. The animals died in periods varying from forty-nine to two hundred and seventy-eight hours. Congestion and inflammation of important organs were produced, as after injection of urea into the blood, or during the course of Bright's disease. It was also seen that "so long as vomiting and purging continued, there was no accumulation of urea in the blood, and consequently no uræmic intoxication."

In the third series. the kidneys were removed, and urea or urine was injected into the blood, which induced death with all the symptoms of uræmia, in from eight to fifteen hours; and when urine was injected into the circulation death occurred in a shorter time than when simple urea (though to a greater amount) was used in solution, whence it is inferred that urea is not the only poisonous element in urine. From his experiments and observations, Dr. Hammond arrives at the following conclusions:—

1. The injection of urea, in limited quantity, into the blood of animals, produces a certain amount of disturbance in the nervous system, similar in its symptoms to the first stages of uræmia, but that this condition even disappears, if the kidneys are capable of so depurating the blood as to eliminate the toxic substance.

2. Urea, when introduced into the circulation in larger quantity than can in a limited period be excreted by the kidneys, induces death by uræmia.

3. By ligature of the renal arteries, or removal of the kidneys, the elements of the urine being retained in the blood, render this fluid unsuitable to the requirements of the organism, and, consequently, induce a condition of system not essentially distinguishable from the uræmic intoxication of Bright's disease, or that caused by the direct introduction of urea into the blood. As, however, was pointed out by Bernard and Barreswil, so long as the urea, or the products of its metamorphosis, are discharged by the stomach or intestines, uræmia does not take place, but, that when these channels become closed, convulsions and coma are produced, and death soon follows.

4. The introduction of urea or urine into the circulation of animals, the kidneys of which have been ablated, shortens the life of such animals, as Frerichs and others have already shown.

5. There is reason to believe that the urine, as a whole, is more poisonous than a simple solution of urea, for, in those cases in which urine was injected into the blood, the amount of urea thus introduced was much smaller than that previously thrown in, in a pure state, and yet symptoms of as great intensity followed.

6. Urea, or the elements of the urine, as a whole, induce such a condition of the nervous system, as strongly to predispose to congestion and inflammation of the viscera, especially the lungs, pericardium, and spleen.

7. Urea, when directly injected into the blood, or suffered to accumulate in this fluid by extirpation of the kidneys, deranges, in some manner, the process of sanguification, so as to disturb the normal relation of proportion existing between the white and the red corpuscles, and either to hasten the decomposition of these latter, or to interfere with the due removal from the blood of such as are broken down and effete.

8. There is no reason to suppose that, under the circumstances specified, urea undergoes conversion into carbonate of ammonia, but that, on the contrary, there is sufficient evidence to warrant the conclusion that no such process ensues. The fact that in the foregoing experiments a larger amount of urea is generally found in the blood taken from the body after death, than in that abstracted during life, is, of itself, conclusive against any such hypothesis.—*British Med. Journal, May* 25, 1861, *p.* 550.

30.—THE URINE IN ALBUMINURIA.

By Dr. Lionel S. Beale, F.R.S.

"If the quantity of albumen be small, amounting to milkiness or opalescence when heat is applied, or nitric acid added to the urine, and especially if the urine be pale and of specific gravity 1012 or lower, we should suspect that the lesion giving rise to the escape of the albumen was chronic, and would ultimately destroy life.

"If the proportion of urea to the other constituents of the solid matter were large, we should form a more favourable opinion than if the per-centage in the solid matter were very much less than in health. In the latter case, a great part of the renal structure would probably be involved; but in the former there would be reason to think the disease had only affected a certain number of the secreting tubules. There are, however, some exceptions to these general statements. Patients have passed small quantities of albumen in the urine for many months, and it has afterwards disappeared. In other cases, the progress of the disease is exceedingly slow. I have known a man

pass urine of the character above mentioned for upwards of twelve years; and I believe this might go on for twenty years, or even longer, the patient perhaps dying at last of some other malady."

In testing for albumen in urine, Dr. Beale reminds us that, in certain specimens, nitric acid occasions a cloudiness from the precipitation of uric acid or an acid urate, quite independently of albumen. The former may be distinguished from the latter either by boiling, which dissipates the urate, or by the microscope, which reveals crystals of uric acid in it. It is, however, quite easy to tell the difference between them without the aid of either means. If the test-tube containing the urine be held in an inclined position, and the acid allowed to trickle slowly down its sides, the heavier liquid will sink through the urine, and form a distinct layer at the bottom of the tube. If albumen be present, a cloudy zone will be produced *immediately above and in contact with* the nitric acid; but if no albumen exists in it, but an excessive proportion of urates, a cloudy zone will appear *at the surface of the liquid*, while the lower part—that in contact with the nitric acid—will remain perfectly clear.—*British Medical Journal, June* 8, 1861, *p.* 608.

31.—ON A NEW CLINICAL METHOD OF ESTIMATING THE QUANTITY OF SUGAR IN DIABETIC URINE.

By Dr. William Roberts, B.A., Physician to the Manchester Royal Infirmary.

[The following paper by Dr. Roberts is highly interesting, and will enable the busy practitioner to ascertain the quantity of sugar in diabetic urine, in a very short time. It is quite true that when a man has a long round of patients, he has not time to perform many of the minute operations which would enlighten him as to the nature of the cases before him; but when a thing can be done easily and rapidly, it will be done much more frequently.]

The only instrument required is the urinometer, an instrument with the use of which every practitioner is familiar, and especially so in this very complaint, diabetes. The principle of the method is most simple. It is, to take the specific gravity of the urine before and after fermentation, and, from the loss of density occasioned by the conversion of sugar into carbonic acid and alcohol, to calculate the amount of sugar destroyed.

I would suggest that this be called the CLINICAL METHOD of estimating sugar, in order to distinguish it from another and older fermentation process, the principle of which is to determine the weight of carbonic acid evolved in fermentation, and therefrom to calculate the quantity of sugar broken up.

The analysis of a diabetic urine by the clinical method is thus performed:—About four ounces of the urine are poured into a twelve-

ounce phial, and a lump of German yeast, of the size of a small walnut or so, is added. The phial is then loosely corked, or covered with a slip of glass, and placed on the mantel-piece, or other warm place to ferment. Three or four ounces more of the same urine are likewise put up tightly corked in a *companion phial*, without any yeast, and placed beside the *fermenting phial*.

Fermentation soon begins, and proceeds with such rapidity, that in twenty-four hours not only is the process completed, but the froth and scum are dissipated from the surface, and the urine, though still cloudy, is in a fit state for the second step in the process,—namely, taking the densities. It is advisable, however, to remove the phials from the mantel-piece a couple of hours beforehand, so that their contents may cool down to the general temperature of the room.

The specific gravity of the two products is then separately observed in the usual way,—by pouring the urine into a cylindrical glass vessel, and dropping in the urinometer. The fermented urine will be found to have suffered a very great diminution of density, ranging from thirty to forty degrees—more or less, according to the amount of sugar destroyed. This diminution of density holds such proportion to the sugar originally present in the urine, that, for each grain of sugar per fluid ounce, there is a fall of one degree of density; or, in other words, *for every degree of density lost, you may count one grain per ounce of sugar in the urine.*

The following example will serve to illustrate the proceeding:—

Density of a diabetic urine before fermentation,	1040 degrees.
Density after fermentation, ...	1002 ...
Density lost by fermentation	38 ...

These 38 degrees of 'density lost' indicate that the urine contained exactly 38 grains of sugar per fluid ounce, or 740 grains per imperial pint. In ten minutes—five minutes one morning and five minutes the next—an accurate quantitative analysis can thus be made. Indeed, the preliminary part of the operation may be left to the patient himself or his attendant. There is no skill required to set aside a few ounces of urine in a twelve-ounce bottle, with a lump of German yeast, and to place beside it a 'companion phial' of the same urine without yeast. The medical attendant need only perform the second part of the operation—the taking of the densities; so that, with five minutes a-day, he may keep an exact register of the diurnal separation of sugar by a diabetic patient.

The reason why so large a phial is required for so small a quantity of fermenting urine is, that sometimes a great deal of froth and scum are produced, and the contents overflow unless plenty of room be provided. The cylindrical glass used for taking the densities does not answer for a fermenting vessel, because the greater depth of the liquid in it impedes and protracts the fermentation. Its wide mouth

is likewise less easily managed than the narrow mouth of a bottle. A vent for the escape of the carbonic acid has to be provided, but evaporation of water and alcohol must be kept at the lowest possible point.

The solid German yeast is superior to brewers' barm for the present purpose, inasmuch as it interferes less with the original density. German yeast yields scarcely any soluble matter to water or urine, and consequently has no sensible effect on the density. The quantity used should be in great excess, in order to quicken the fermentation; but a little more or a little less does not perceptibly sway the results. In a trial carefully made, with a view of testing this point, I found that 96 grains of yeast added to a 1000 grains of saccharine urine depressed the density, through fermentation, from 1036·40 to 1001·12, showing a loss of 35·28 degrees. The same urine, with 192 grains of yeast, or twice the quantity, lost 35·20 degrees. So that although twice as much yeast was used in the second as compared with the first experiment, the result was so nearly alike in the two cases that they may be regarded as identical.

A somewhat more important matter than the quantity of yeast is the degree of clearness attained by the fermented urine at the time when its density is taken. A fermented urine remains turbid for many days after the cessation of fermentation, and a sediment gradually accumulates at the bottom of the vessel as the floating sporules of the yeast plant subside. As the liquid clears, there is a progressive, though slight, diminution of its density. In twelve hours the density falls, from this cause, about a quarter of a degree, and in twenty-four hours it falls from two thirds to half a degree. Error from this source, at the worst, is of no great moment; and it may be reduced to insignificancy, or altogether avoided, by taking the density always at about the same time after the cessation of fermentation. With an adequate supply of yeast, and a sufficiently warm situation, fermentation is usually completed in about twelve hours, and the remaining twelve hours of the day suffice for the dissipation of the froth and scum. It is therefore advisable, for the sake of accuracy as well as convenience, to take the density of the fermented urine about twenty-four hours from the commencement of the experiment.

The use of the 'companion phial' is not, of course, an essential feature of the analysis; but it affords two not inconsiderable advantages. It does away, in the first place, with the necessity of making a note of the specific gravity of the urine the day before, and permits the two densities to be taken at the same moment and submitted to immediate comparison. But a far more important advantage is, that it provides a sure and easy means of obviating inequalities of temperature, which would otherwise seriously compromise the exactness of the results. A variation of temperature of 10° F. affects the specific gravity about one degree; so that, if the temperature of the fermented urine were 20° higher or 20° lower than that of the unfermented

urine at the time when the densities were taken, an error of two grains per ounce would creep into the analysis. And if the densities were taken on successive days, a variation to this extent might easily arise from oscillation of the external temperature, or of the heat of the room. But if the two urines—the fermented and unfermented—be kept at the same temperature, and they would be so standing side by side, the difference between their specific gravities would remain constant, whether that temperature were 70° or 40°, and the accuracy of the analysis would be maintained.

The ordinary urinometer answers very well for the determination of the gravities. It is advisable, however, to choose an instrument with a long scale, for easier reading. If more accurate results are desired, the scale may be divided into two parts on separate instruments. I have had constructed for my own use two urinometers, the scale of each ranging through 30°—one from 995 to 1026, and the other from 1025 to 1055. The degrees are so far apart that, in a clear urine, the density can be read to a quarter of a degree.

The clinical method is susceptible of very great accuracy. Indeed, if the densities are taken by the specific gravity bottle, at the temperature of 60°, it scarcely yields to the volumetrical analysis, as may be seen by a glance at the following tables. The first table places in comparison twenty observations made by the two methods on various diabetic urines, with densities ranging from 1031·52 to 1053·48.

No.	Sugar per 100 parts by the Clinical Method.	Sugar per 100 parts by direct Volumetrical Analysis.	Difference.
1	7·51	7·69	0·18
2	7·47	7·69	0·22
3	6·68	6·66	0·02
4	6·72	6·66	0·06
5	5·16	5·18	0·02
6	5·19	5·18	0·01
7	5·65	5·77	0·12
8	5·65	5·77	0·12
9	4 47	4·35	0·12
10	4 49	4·35	0·14
11	7·85	8·06	0·21
12	5·91	6·10	0·19
13	11·27	11·36	0·09
14	11·21	11·36	0·15
15	5.69	5·68	0·01
16	8·11	8·06	0·05
17	8·09	8·06	0·03
18	8·00	8·06	0·06
19	8·29	8·20	0·09
20	7·61	7·74	0·13

These results are so close, that the discrepancies may be considered as within the limits of unavoidable error.

In order yet further to test the constancy of the results the method was subjected to a still severer trial. A natural diabetic urine was chosen, and its per-centage of sugar accurately ascertained. Various dilutions of known strength were then prepared from it, both with water and with healthy urine. Assuming the estimate of sugar in the original urine as correct, the per-centage of sugar in the dilutions could be ascertained with almost absolute correctness by a simple calculation. These dilutions were then fermented, and their loss of density compared with the known proportion of sugar contained in them.

The following table exhibits the results:—

	Sugar per 100 parts by the Clinical Method.	Sugar per 100 parts by calculation from the first estimate.	Difference.
1. A natural diabetic urine,	5·91	...	...
2. The same, mixed with $\frac{1}{10}$ of its bulk of water,	5·31	5·32	0·01
3. The same, mixed with $\frac{2}{10}$ of its bulk of water,	4·71	4·73	0·02
4. The same, mixed with $\frac{3}{10}$ of its bulk of water,	4·16	4·14	0 02
5. The same, mixed with $\frac{1}{10}$ of its bulk of healthy urine,	5·34	5·32	0·02
6. The same, mixed with $\frac{2}{10}$ of its bulk of healthy urine,	4·77	4·73	0·04
7. The same, mixed with $\frac{3}{10}$ of its bulk of healthy urine,	4·15	4·14	0·01
8. The same, mixed with $\frac{9}{10}$ of its bulk of healthy urine,	0·70	0·59	0·11

Numbers so nearly alike as those in these two columns may be regarded as practically identical. In the last experiment only, where the quantity of sugar was under one per cent., was there a sensible discrepancy.

Solutions of cane sugar in water and in healthy urine were subjected to similar trials, and yielded equally satisfactory results.

In the foregoing part of the paper. the result of the analysis has been brought out as so many grains in the fluid ounce; and from the

simple relation existing between the 'density lost' and the number of grains of sugar per ounce, this is evidently by far the most convenient formula for clinical purposes. If it be desired to bring out the result as so much per 100 parts, the following formula expresses the amount of sugar in those terms. Let D be the density of the unfermented urine, and D′ the density of the fermented product; then D — D′ expresses the density lost in fermentation. By multiplying the 'density lost' into the co-efficient 0·23, the product represents the sugar per 100 parts. That is, sugar per cent., or $S = D - D' \times 0{\cdot}23$.

The successive steps of the analysis may be thus recapitulated :—

1. Four ounces of urine are placed in a twelve-ounce phial, with a lump of German yeast of the size of a small walnut.

2. This is loosely corked, or covered with a slip of glass, and placed in a warm place to ferment.

3. A companion phial filled with the same urine—say a three ounce phial—is tightly corked, and placed beside the fermenting phial.

4. In about twenty-two hours, when fermentation has ceased, the two phials are removed, and placed in some cooler part of the room.

5. Two hours after—that is, about twenty-four hours from the commencement of the experiment—the contents of the phials are separately poured into cylindrical glasses, and the density of each observed.

6. The difference between the two densities is thus ascertained, and every degree of 'density lost' indicates one grain per fluid ounce of sugar in the urine.—*Edinb. Med. Journal, Oct.* 1861, *p.* 326.

32.—*New Test for Saccharine Urine.*—This test, which possesses extreme facility of application, and is stated by the author to be of great delicacy, depends upon the simple conversion of the sugar into caramel. It is thus described :—"Upon a clean slip of tinned iron, place one or two drops of the suspected material, and hold it over a spirit lamp: the fluid will speedily evaporate, leaving, if the process be arrested at that point, scarcely a trace upon the metallic surface. Continue the application of heat; in a few moments after the dessication is complete, a spot of an inch or so in diameter, over which the drop had spread with the first ebullition, will gradually assume a rich reddish-brown colour, with a brilliant lustre, as if coated with a film of varnish or Japan lacquer. A strong heat produces a darker colour, but the lustre continues till the heat becomes sufficiently intense to decompose the substances."—*Southern Med. and Surgical Journal; and London Med. Review, May* 1861, *p.* 569.

33.—ON ANASARCA.

By Dr. Charles Witt, Spring Gardens, London.

The following method of relieving that well-known result of internal disease—anasarca, may prove of interest to the profession. It is, to say the least, well worth a careful inquiry, whether something cannot be done, however temporarily, to effect the removal of the water, and thus to save our patients much of the alarm and annoyance of this addition to their sufferings.

Let me premise, that there appears to be nothing in anasarca partaking of the character of a curative effort of Nature, it is usually the consequence of a mechanical obstruction to the circulation, and experience has not yet shown whether there is any valid objection to such consequence. Medicines have been plentifully administered with this object, but, it is well known, with no satisfactory results; and I venture to hope, therefore, that in the simple process here submitted means of relief will be found. By an easy and painless operation (founded on the structure of the tissue where the mischief lies) the whole of the serum can be quickly discharged. The way in which this may be accomplished will be best shown by detailing a case and its treatment:—

A lady in Northamptonshire had long suffered from anasarca, the result of heart-disease. From the chest downwards the body was so much distended with fluid that for many months she had been unable to lie down, but was obliged to sit day and night on a chair. On account of her size and weight, every requisite movement was effected with difficulty; her breathing was short and laboured: to be brief, her death was almost from hour to hour expected. At a consultation on the case, Dr. Robertson (consulting physician to the Northampton Infirmary), Mr. Dix (of Long Buckby), and myself being present, the following mode of relief was adopted in the utter hopelessness of any other:—Each leg was placed in a separate foot-bath, when a lancet was deeply inserted into the mass of cellular tissue, which projected most on the outer side of each ankle. A stream of water instantly began to flow from both apertures, and continued to do so with such volume and rapidity that in less than twelve hours the whole of it was removed, and the body and legs had assumed their natural size and form. Bandages of flannel were repeatedly applied as the diminishing size required, and were kept tight about the body as a precaution against faintness. The relief to the patient was immediate and gratifying, and for the first time for several months she lay down on her bed and slept comfortably. There was, of course, a slight oozing from the apertures; but these, being kept closed by adhesive plaster and bandages, readily healed—almost as soon, perhaps, as they would have done in a patient otherwise healthy. The operation was subsequently performed by Mr. Dix six or seven times during the remainder of the lifetime of this patient, which lasted about five months after

the first administering of relief, and the water was never again allowed to accumulate so as to cause inconvenience.

I trust that a satisfactory proof is here given of what may be safely done, even in the advanced stage of anasarca; and I submit that the same treatment will prove efficacious in its earlier stages by the removal of smaller accumulations of water, while the operation may be repeated as often as is required. Some faintness occurred after the first operation in the above case; indeed, the loss of a large quantity of fluid which has so long pressed on the blood-vessels must inevitably cause this tendency. Hence arises the necessity of keeping a bandage round the body; and it is necessary also that the patient should keep strictly quiet for some time. As to erysipelas following this operation (the probability of which has been suggested by one or two of my medical friends), I am not of opinion that this is much to be feared. Let me add, that if patients afflicted with anasarca are to left without artificial aid in the discharge of the serum collected—what follows? Nature attempts it, in a somewhat similar, but ineffectual, manner; the distension goes on increasing, till at last the legs burst open by ulceration, and then only a partial escape of the fluid takes place; no real relief ensues, not to speak of the pain of so tedious a process. In the majority of cases, I believe that certain relief may be given, as well as life prolonged.—*Lancet, Oct.* 19, 1861, *p.* 370.

SURGERY.

AFFECTIONS OF THE BONES AND JOINTS, ETC.

34.—ON AMPUTATIONS.

By Thomas P. Teale, Esq., Surgeon to the Leeds General Infirmary.

[In an interesting paper on this subject, chiefly referring to amputations at the Leeds Infirmary, Mr. Teale makes some remarks which are valuable. He finds that in traumatic cases the secondary amputations were much more fatal than the primary; and, that his rectangular operation had the advantage over other methods. This favourable result he attributes to the more complete covering of the end of the bone, and to the avoidance of irritation and distress in the subsequent treatment. He says,]

In Hospitals, when erysipelas and pyæmia are prevailing, stumps frequently "open out," suppurate, or become phagedenic. But when rectangular stumps are thus affected, and life is preserved, the resulting stump is, with very rare exceptions, a good one. In several cases, apparently most unpromising, although the stump has lain long without showing a disposition towards repair, yet when recovery was at length accomplished, there was a soft moveable mass of tissues over the end of the bone, almost as satisfactory as in the cases which had done well from the beginning.

After ordinary amputations it is admitted by surgeons of great experience, that exfoliation and necrosis are *frequent*. After rectangular amputations they are *rare*. After 103 of these operations, in two cases only, and to a very small extent, have exfoliation or necrosis come under the knowledge of my colleagues or myself.

In some of the cases of rectangular amputation published in the journals, sloughing of the long flap is occasionally noticed. Our Hospital records show that it is about equally frequent but less extensive in this as compared with other modes of operating. The following short extracts from the hospital note-book show all the notices which it contains of sloughing of the stump. It thus appears that in 103 rectangular operations, sloughing occurred to a greater or less extent, in 8, as follows:—

1. Thigh, pathological, one inch of slough.
2. Leg, traumatic, one corner.

3. Thigh, pathological, one inch and a-half slough, excellent stump.
4. Leg, pathological, a little sloughing.
5. Leg, pathological, a little sloughing.
6. Arm, traumatic, some sloughing, good stump.
7. Leg, pathological, sloughing of considerable portion.
8. Leg, traumatic, considerable sloughing.

In 86 amputations, not rectangular, sloughing is recorded in 6, as follows:—

1. Thigh, pathological, double flap, some sloughing and exfoliation.
2. Thigh, traumatic, circular, considerable sloughing and sequestrum.
3. Leg, pathological, double flap, some sloughing.
4. Leg, traumatic, irregular, considerable sloughing.
5. Arm, traumatic, irregular, sloughing.
6. Forearm, traumatic, circular, extensive sloughing, and bone exposed.

From these records it appears that, so far as the cases at the Leeds Infirmary are concerned, sloughing is a more serious affair in the general amputations than in the rectangular. It should also be stated, in reference to the rectangular operation, that, in consequence of its being a new proceeding, minute deviations from a satisfactory state were noticed; whereas in the other and earlier operations, slight degrees of sloughing have probably occurred, and have not been thought worthy of notice.

As far as sloughing in rectangular amputation of the leg is concerned, a circumstance may be mentioned which has, I believe, in some instances, been the cause of it. I allude to the wounding of the anterior tibial artery at the base of the flap. This accident may be avoided by observing the following rule. The long anterior flap having been marked out by carrying the two lateral incisions through the integuments only, and the transverse incision through all the structures down to the bone, the aponeurosis covering the muscles in front of the leg should next be divided throughout the whole length of the flap close to the edge of the tibia on one side, and of the fibula on the other. This being done, there is little need for the further use of the knife, as the finger is almost sufficient to detach the mass of muscles upwards from the interosseous ligament, more especially in the situation of the artery, a few touches of the knife being only required laterally to separate the muscles from the bones.

The most marked advantage of the rectangular amputation is seen in the character of the stump produced by it. The perfection of a stump may be tested by its ability to bear pressure on its extremity, and thereby to favour free locomotion. I can with confidence state that, with scarcely an exception, in the cases under the observation of the surgeons of the Leeds General Infirmary, these stumps allow of a firm bearing of their extremity on the bottom of the socket of the

artificial limb, and, consequently, give to the patient great stability and confidence in walking; while the circular and double-flap stumps rarely can bear pressure on their extremity, and the subjects of such stumps have to remain contented with the uncertain support which is derived from the apparatus resting on the head of the tibia, or on the tuberosity of the ischium.

On the grounds which have now been stated, namely, the somewhat lessened mortality, the greatly diminished suffering in the after treatment, and the vastly superior condition of the stumps, so far as comfort and usefulness are concerned, my colleagues and myself during an experience of six years have adopted the rectangular as our general mode of amputating, deviating from it from necessity, and not from choice, in such cases only in which the extent of the injuries or the disease renders the rectangular amputation impracticable or undesirable.

The following table of the two classes of amputations, as practised during each of the eight years embraced in this record, will show the extent to which the rectangular has, in our estimation, superseded the other methods of amputating:—

	Amputations not Rectangular.	Rectangular Amputations.
1853	23	—
1854	22	—
1855	13	6
1856	8	11
1857	9	21
1858	2	19
1859	2	16
1860	7	30

—*Med. Times and Gazette, July* 6, 1861, *p.* 6.

35.—ON THE INFLUENCE OF REST IN THE TREATMENT OF DISEASES OF THE JOINTS.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital, &c.

When a joint becomes inflamed, it is painful and difficult of movement; it becomes involuntarily fixed by Nature's own process, thus securing comparative rest to the interior of the joint. Indeed, we may lay it down as an axiom, that Nature instinctively renders an inflamed joint comparatively fixed and flexed. Now, what is the key to the explanation of the flexed position and rigid state of an inflamed joint? Some might say, "Because the patient finds it the easiest position for himself, and the easiest position must, of course, be the best." But this is no answer to the question, How is this position induced? It is not voluntary. The patient cannot either prevent it or change it when it exists; nay more, it is not true that the flexed

position is the easiest; for every day's experience declares the contrary, and I shall hope to have the opportunity of showing you distinctly that, when the hip-joint is diseased, it is flexed, and it is often in a very painful state; but that when the femur is put straight so that the flexed condition is destroyed, the patient from that time is free from pain. Here is an instance in direct contradiction of the idea that the flexed condition is the easiest for the patient. When the joint is made straight, the patient experiences, almost immediately, a diminution of pain. It is true, however, that in the case of the knee-joint it is sometimes otherwise; the reason for which I will endeavour to point out, through the medium of these diagrams, which represent the knee-joint in an extended, semi-flexed, and completely flexed positions. It is plain that when the femur, tibia, and patella are in the position in which you here see them, the leg being in a state of extension, there must be a much larger extent of articular surface coaptation than in any other position of the joint; and this surface coaptation becomes more and more reduced, or less in extent, as the tibia is carried backwards away from the femur; and, probably, it is this diminution in the extent of the articular surface contact, as well as the diminished pressure, which explains the feeling of comfort that patients sometimes experience when the inflamed knee-joint is allowed to become flexed.

Still the question comes to us, Why is an inflamed joint fixed and flexed? It appears to me to result from the circumstances to which I shall allude presently more in detail—viz., that the irritated or inflamed condition of the interior of the joint (say the knee-joint), involving the whole of the *articular nerves*, excites a corresponding condition of irritation in the same nervous trunks which supply both sets of muscles, extensors, and flexors: but that the flexors, by virtue of their superior strength, compel the limb to obey *them*, and so force the joint into its *flexed condition*. The joint becomes rigid and flexed then, because the same nerves which supply the interior of the joint supply also the muscles which move the joint.

This anatomical arrangement, physiologically considered and rightly interpreted, may be made the means of explaining some of the most prominent symptoms of joint diseases, and of constituting a foundation for the required treatment of diseased joints by rest, which I propose to advance and to recommend.

In order to place this important anatomical, physiological, and pathological subject comprehensibly and definitely before you, I will state it thus: "*The same trunks of nerves whose branches supply the groups of muscles moving a joint furnish also a distribution of nerves to the skin over the insertions of the same muscles; and—what at this moment more especially merits our attention—the interior of the joint receives its nerves from the same source;*"—thus implying an accurate and consentaneous physiological harmony in those various co-operating structures.

The large drawing which is placed before you was taken from a recent dissection, kindly accomplished for me by Mr. Durham, of Guy's Hospital, and depicted by an artist (Mr. Tupper) not now attached to Guy's School. You may see here the circumflex nerve branching into the teres minor and the deltoid muscles, distributing itself to the skin over the deltoid, and then proceeding to the skin over the fascia of the upper arm, to which the deltoid muscle is partly affixed. Yonder will be noticed the articular branch of the circumflex nerve, the branch which proceeds to the shoulder-joint. Other nerves go to that joint, such as a branch from the subscapular trunk, which is derived from almost precisely the same part of the axillary plexus as the circumflex nerve. So the suprascapular nerve, after supplying the supraspinatus and infraspinatus muscles, sends little filaments to the interior of the same joint. Thus the same circumflex nerve supplies these muscles, the joint which these muscles move, the skin over the muscles, as well as the skin over the fascia, which is a part of the insertion of the deltoid muscle. But here I touch upon a point, by which I am reminded to state that every fascia of the body has a muscle attached to it, and that every fascia throughout the body must be considered partly as the insertion of a muscle. I shall be able hereafter to trace nerves derived from the same trunk supplying the joint and muscle, sometimes distributed over a large extent of fascia. That extent of nerve distribution, however, I shall show to have a very exact relation to the proportionate attachment of the muscles to the fascia, of which I shall be able to adduce some well-marked examples.

The object of such a distribution of nerves to the muscular and articular structures of a joint in accurate association, is to insure mechanical and physiological consent between the external muscular or moving force, and the vital endurance of the parts moved—namely, of the interior joints (whether it be of friction or of pressure); thus securing in health the true balance of force and friction until deterioration occurs. If this point of balance or adjustment be overreached during exertion, pain, Nature's warning prompter, is induced within the joint, and suggests the necessity of diminishing or arresting exertion. This cessation or this reduction of exercise, or friction and pressure upon the articular structures, *must be effected* by the muscular apparatus of the joint, either through the will, or immediately by its own instinctive efforts, called into play by means of the nervous association. The muscles, indeed, appear to be told, through the medium of the nerves of the interior of the joint, that its articular structures are overtaxed; and, the antagonistic muscular forces of the joint being thus involuntarily excited, it (the joint) is at once rendered rigid and stiff, for the purpose of keeping it at rest. Or it may be put in this way (only as confirmatory, however),—that the interior of the joint itself, under the influence of physiological exhaustion, contributes something towards the same end, by failing to secrete the proper

quantity of synovial fluid. The joint thus robbed of its lubricating medium demands of the *exhausted muscles*, for its movement, a still greater effort than in its normal state; and their inability to respond to the extra demand necessitates the quietude or rest of the joint. Still, in this mode of viewing the subject, the necessity for limiting exertion, in order that the interior of the joint may obtain the congenial rest requisite for the renewal of its physiological power and structural integrity, is intimated to the moving agents by the same nerve interpreters, and they (the moving agents), in obedience to the announcement, render the joint stiff and difficult of movement.

Without this muscular and articular nervous association in joints there could be no intimation, by the internal parts, of their exhausted function; and there cannot be any doubt that it is when this period of functional exhaustion in the internal parts has been reached, and articular friction is nevertheless continued (notwithstanding the structural and functional prostration,) that the mischief to the articular structures commences, and disease of the joints, as we term it, starts into existence.

Again, through the medium of this *muscular and cutaneous* nervous association, great security is given to the joint itself, by those muscles being made aware of the point of contact of any extraneous force. Their involuntary contraction instinctively makes the surrounding structures tense and rigid, and thus is brought about an improved defence against further temporary encroachment.

This same associated muscular and cutaneous nerve distribution is obviously of the highest importance in reference to the sense of touch, through a recognition induced by due pressure. In the absence of this peripheral, or cutaneous, and muscular harmony, through the medium of the common distribution of the same nerve, the refinements of the sense of touch could scarcely be acquired; for rude or heavy pressure is incompatible with minute and delicate appreciation by touch. So that if I touch anything with my forefinger, the impression made upon the sensitive nerve conveys to the associated motor nerve from the same trunk the precise extent of pressure which is necessary for the recognition of the required force. In prehension, or grasping with the hand, the indication as to the completion of the necessary force to be employed must be the impression made upon the sensitive branches of the same nerve which supplies the muscles employed in the act of prehension or grasping. Thus the median nerves, with others, are employed in grasping, and in the adaptation of the fingers to the palm of the hand. The limit of that grasping is the perfect recognition of the contact of the different parts of the hand to be pressed upon. Whether the body to be grasped be great or small, there must be an instinctive limit to the grasping effort. This can only be indicated to the muscular apparatus employed by the sensitive nerves. So that I say, in prehension, the indicator as to the completion of the necessary force to be employed must be the

impression made upon the sensitive branches of the same nerve which supplies the muscles employed in the act of prehension or grasping.

So in standing, walking, or jumping. The contact of the skin of the foot with the ground intimates, in health, only the degree of muscular force necessary to sustain the erect posture in standing; to guage the use of only the required energy in walking; and to moderate the concussion of jumping. Without this normal, consentaneous muscular and sensitive function, precision of action would be lost, and unmeasured exercise of muscular force would be employed during the performance of any of these functions. The nice adjustment of muscular power must be an important thing, for we know that unexpected muscular force breaks the patella.

Now let us go to the bedside. Is it not indisputable that if we allow an inflamed knee-joint to lie upon a soft pillow, unconstrained by mechanical means, the muscles of the joint, stimulated to undue exertion, never allow the articular surfaces to be kept quietly in apposition without pressure upon each other?—thus explaining the fixed condition of the joint. The flexion, too, receives its explanation from the exalted muscular vigour. Thus the flexors—as I before remarked, the more powerful muscles—act unceasingly day and night, apparently without any rest, and especially declare their mischievous assiduity by the wakeful slumbers and disturbed sleep of the patient. This persistent action of the flexors slowly alters the relation of the articular surfaces of the bones to each other—more slowly indeed, but not less surely, than the hour-hand of that clock, which does not appear to move, but yet if I compare its present position with that which it occupied when I commenced this lecture, I discover that it has gone on marking the course of time.

Let us take the case of the knee-joint again. The surgeon sees the inflamed and swollen joint, lying on its outer side, and desires that it may be kept quiet upon the bed, or on a soft pillow. Whether he sees it again in a few hours or days, or in a week, the first thing that strikes him with regret, and perhaps surprise, is the changed position of the bones. He sees that the knee-joint is considerably more flexed than when he saw it last. On inquiry, he ascertains that neither the patient nor the nurse has disturbed it, and then he recognises that the stealthy and mischievous flexors have secretly effected this change in position. If you give the patient chloroform, and destroy muscular action for a time, the joint is then perfectly moveable by ordinary manual force; so that it is not the mechanical distension of the *interior* of the joint by fluid, or the incumbrance of the joint by the fluid or solid results of inflammation upon its *exterior*, which produces early rigidity or flexion: it is forced coaptation and flexion under the influence of muscular energy involuntarily exerted; for as soon as muscular contraction is destroyed by chloroform, the fixed and flexed conditions of the joint are for the time entirely lost. And here I may state, that over and over again I have seen the mis-

chievous and destructive effect of this increasing state of flexion of the joint, and not seldom, I may say, resulting from procrastination, or inexcusable want of courage on the part of the surgeon to rectify it, or from his yielding too easily to the importunities of the patient or his friends, in order to avoid giving pain to the patient. From the time when the limb begins to be flexed starts the mischief, which by-and-by cannot be controlled.

It seems to me that a great mistake is made on the part of professional men, when they see an inflamed knee or other joint beginning to be contracted or flexed, not to correct it at once. I believe that their duty lies in preventing the increase of the flexion, and so averting the condition, which, in some cases at all events, leads to the necessity for amputation.

This sole explanation of the cause of the fixed and flexed joints applies particularly to recently inflamed joints, before the deposition of new and comparatively solid material has taken place around and within the articulation, such as usually occurs in cases of long continued or chronic inflammation of joints, when, no doubt, the contraction and fixed condition depend partly on the incumbrance of the joints by the new material, and hence the deformity of the joint at that period cannot even under the influence of chloroform, be entirely removed.

And here let me just refer again to this constantly flexed state of an inflamed joint. Take an inflamed hip-joint: I venture to say that no gentleman here ever saw an inflamed hip-joint with the leg extended. It is uniformly bent, and, also, as a rule, slightly adducted, the cause of which I shall have occasion to refer to at another time. In the case of the knee-joint, when inflamed it is always flexed; but, curiously enough, the malingerer, willing to deceive and to impose, almost always endeavours to indicate his long continued and extreme suffering by fully extending the leg. But this extended position displays the imposition, for an inflamed joint is never straight, but always flexed, the degree of that flexed condition depending upon the intensity or the long duration of the mischief. Did any person ever see an inflamed ankle-joint, permitted to pursue its own course, remain nearly at a right angle with the foot? The heel is always raised by the gastrocnemii, and the toes pointed downwards. So with an inflamed elbow-joint; it is always flexed, never extended. But when we come to the wrist-joint, we find that, although the muscular power associated with it is very great, yet in consequence of the extending and flexing forces being pretty equally balanced, we seldom have much flexion of the hand when the wrist-joint is inflamed. This is in strict accordance with the law which I have endeavoured to place before you —the quality of muscular force not causing much disturbance of the bones, but simply maintaining the joint in a quiet, but fixed condition. So again with the fingers. Who ever saw a finger with an inflamed joint of any duration in which the finger was perfectly straight? Such a thing was never seen; the inflamed joint is always more or

less bent under the influence of the flexors. I think we may in the hand recognise the proportionate condition of flexion associated with diseased joints, in reference to a distribution of nerves, on which I shall have occasion to dwell at our next meeting. The forefinger, for example, when inflamed, is generally not so much flexed as the others, because it has an added muscle, the extensor indicis, which counteracts the tendency to become flexed. With regard to the thumb, I should say, as a rule, it is not proportionately so much flexed towards the palm when inflamed as the middle or ring finger. The explanation lies in this: that there is a greater balance of power in the muscles of the thumb, the flexors and extensors, as compared with the corresponding muscles of the fingers. This will explain the extreme flexed condition of some of the fingers, the slight degree of flexion of the forefinger, and the nearly straight condition, as frequently observed, in an inflamed joint of the thumb.—*Lancet*, *July* 20, 1861, *p.* 57.

36.—ON LOOSE CARTILAGES IN THE KNEE-JOINT.

By W. Joseph Square, Esq., Surgeon to the Devon and East Cornwall Hospital, &c.

[Mr. Syme and others have been accustomed for some time to remove these bodies from the joints by subcutaneous operations. Mr. Square and his colleague, Mr. Whipple, give us the following interesting cases in confirmation of this practice.]

Case 1.—James Ellis, aged 33 years, a farm servant, of healthy constitution, was admitted into the South Devon and East Cornwall Hospital on the 3rd of December, 1856. He has been lame 18 years, and very decidedly so during the last twelvemonth. He has felt two movable bodies in his right knee-joint for fourteen years. With the movement of the joint they glide from one part to another with extreme mobility, and thereby cause great distress and sudden lameness. The joint is often distended with synovia.

Operation, Dec. 8th. I first fixed the cartilages firmly at the lower and outer side of the joint, and entrusted them to an assistant. Two subcutaneous separations of the cellular tissue were then made with a long tenotomy knife (two inches in length), at two separate points of puncture.

From the lower puncture the point of the knife was directed upon the largest cartilage, and the synovial membrane freely incised upon it. The cartilages were then easily slid along the subcutaneous tract prepared for their reception. They were fixed at the end of this tract with a pad of lint, adhesive plaster, and bandage. A straight splint was applied across the back of the limb. The limb was placed at an angle of forty-five degrees, and cold water dressing applied.

Dec. 22nd. He has experienced no pain, nor has there been any inflammatory action, or synovial effusion. The cartilages were therefore excised.

Dec. 30th. Discharged, cured, with slight stiffness of the joint.

Case 2.—Robert Windsor, aged 27 years. farm servant, healthy constitution. A loose cartilage the size of a walnut is readily felt in the left knee-joint. It gives rise to the ordinary symptoms.

April 28th, 1858. The cartilage being firmly fixed by Mr. Fox at the upper and outer angle of the joint, I punctured the skin with a two-inch tenotomy knife, two inches above the cartilage; by a semicircular sweep separated the cellular tissue from the subjacent fascia, then cut upon the cartilage at right angles to it, freely dividing the joint capsule. The body now eluded Mr. Fox's grasp, and slipped back into the joint. With difficulty I managed to bring it again to the opening in the synovial membrane, and pressed and lifted it into the before described subcutaneous tract. The patient was treated exactly as detailed in Case 1.

May 4th. No pain or inconvenience has arisen; cartilage excised.

May 6th. Discharged, cured; walks well.

Case 3.—Edwd. Mitchell, aged 30 years, farmer, healthy constitution.

May 13th, 1858. Three small foreign bodies are distinguishable in the right knee-joint, the smallest is not as large as a garden pea. By slight pressure they glide with amazing swiftness into various parts of the joint, and can only be fixed with moderate certainty at its inner and lower part, just in front of the internal lateral ligament. There is no thickening of the synovial membrane, nor effusion into the joint. Two years ago he first discovered a loose cartilage in the joint, but he had then been already lame for some months. Two months since he discovered a second, and a fortnight ago a third foreign body. His conviction is that the first discovered cartilage was larger than either now in the joint, whence the inference that the two last discovered were produced by the disintegration of the first.

Operation, May 15th. He sat in a chair as the most ready mode of fixing the cartilages, which was effected with difficulty at the lower and outer part of the joint. The puncture and subcutaneous separation being made as in Case 2, I endeavoured to push the cartilages out of the joint, instead of which they slipped back into its cavity. The patient now after much trouble and manipulation, presented one of the cartilages at the incision of the synovial membrane, and I slid it into its bed. Ten minutes of continuous manœuvre were consumed in getting a second cartilage into the subcutaneous tract, but the third could not be found. The cartilages were retained *in situ*, and the patient treated as before described.

May 21st. The operation has caused no pain or inconvenience. Cartilages excised.

May 28th. Returned home. Motion of joint weak but natural.

Case 4.—Francis Warren, aged 20 years, farm labourer, robust constitution. Two months ago he was suddenly attacked with severe pain in the right knee-joint, with considerable tumefaction. Under surgical care he gradually recovered, but has never entirely lost pain in the part. Three months ago he discovered the existence of the loose cartilage. It is now about the size of an almond in its shell. It is extremely mobile and difficult to fix.

Operation, Jan. 25th. I performed the operation of subcutaneous section, with the intent of embedding the cartilage in the cellular tissues of the thigh, but from the excessive mobility of the cartilage, I failed to accomplish my object. The patient was treated precisely as if the operation had succeeded. No inflammatory action resulted.

Feb. 8th. Operation repeated, with this difference, that the subcutaneous separation was effected over the inner surface of the tibia. I now succeeded well, and lodged the cartilage in the space prepared for it. He was treated in all respects as before described.

Feb. 15th. No inflammatory excitement or uneasiness has occurred. Cartilage excised. Discharged cured.

Case 5.—William Davey, aged 24 years, a healthy farm labourer. His right knee has been weak for some years. He discovered the presence of a loose cartilage nine weeks since, It gives great pain in walking. It is flat, and about the size of an almond.

May 11th, 1860. By subcutaneous incision the cartilage was removed from the joint, and embedded in the cellular tissue, on the inside of the head of the tibia. By pad, adhesive plaster, and straight-back splint, it was retained *in situ*.

May 11th. Cartilage excised.

May 25th. Discharged cured. No inflammatory action has followed the operation. Walks well.

Case 6.—William Davey was again admitted into the South Devon and East Cornwall Hospital on Feb. 5th, 1861. For some weeks he has felt another loose cartilage in the same knee-joint.

Feb. 8th. I endeavoured to perform the usual subcutaneous operation, but failed to dislodge the cartilage. He was treated in the manner already described, and no bad results occurred.

Feb. 22nd. Operation repeated, and again without success. He was treated as before, and with the like immunity from inflammatory action.

March 5th. I discharged him from the hospital, being unwilling to wound the synovial membrane again so soon after two operations.

April 24th, 1861. William Davey was again admitted into the hospital. On the 26th I again performed the operation of subcutaneous incision, but unsuccessfully. His after-treatment was as before.

May 2nd. Allowed to get out of bed. He has had no pain or inflammatory action in the joint since the operation.

May 20th. Discharged for re-admission.

Case 7.—John Ellacott, aged 40 years, a healthy farm servant, was admitted into the South Devon and East Cornwall Hospital, on the 8th of September, 1858, under Mr. Whipple. A loose cartilage, the size of a horse bean, is readily felt in the right knee-joint. He has the ordinary symptoms of the malady. The joint contains no fluid, and its capsule is not thickened. He discovered the cartilage about a year since. It has not increased in size. He never had an accident.

Sept. 10th. The cartilage slipped into the cellular tissue of the thigh, by the ordinary subcutaneous plan.

Sept. 14th. Cartilage excised. It was much smaller than any of the others. No inflammatory symptoms followed the operation. Cured.

Case 8.—Medland, a miner, of good constitution, was operated upon by Mr. Whipple, in 1859. The patient had recognized the presence of a large cartilage for about three years. His symptoms were severe, occasionally he suffered excessive pain, and could not move from this cause, and from faintness, until he had by manipulation altered the position of the cartilage. The operation was performed after the usual manner, and the cartilage was excised on the eighth day. No inflammation followed. He walked four miles the day after the excision of the cartilage, six the next day, and four days after resumed his labour.

Case 9.—Joseph Badcock, aged 39, game-keeper, healthy constitution, admitted into the hospital under the care of Mr. Whipple, April 18th, 1861. He has felt the cartilage (which is now very large) for fourteen years. He has the ordinary symptoms, and is almost *hors de combat* as regards active exertion. There is considerable synovial effusion, and thickening of the joint capsule.

April 19th. Mr. Whipple operated successfully. He displaced the cartilage from the joint and lodged it in the cellular tissue. Patient treated in the ordinary way.

May 5th. Cartilage excised. It was found to be of very irregular outline; its surface nodulated.

May 10th. Patient discharged. He has had no pain, but there is some synovial effusion, and lameness.

June 13th. Is much improved, walks better. Synovial effusion still exists, but is diminished.

Thus, in the treatment of these nine cases of loose cartilage, the knee-joint has been opened by subcutaneous incision thirteen times. It is worthy of remark that neither pain, inflammatory action, nor any serious symptom has in any one instance arisen. The steps of the operation have been alike, the after-treatment the same. A small pad has been placed along the tract of the wound, and upon its orifice, to press the surfaces of the tract together, to prevent hemorrhage, and to retain the cartilage *in situ*. A straight splint has been applied along the back of the limb, to avoid movement of the articular

surfaces. The patient has been confined to bed, with the limb at an angle of 45°, and in the majority of the cases, but not in all, water dressing has been employed.

It would be absurd to say that, by the old method of getting rid of these bodies the knee-joint could not be opened with impunity thirteen times successively; but, it is not too much to state that this is far, very far from the ordinary experience of surgeons. In common justice to humanity, I must urge the adoption of the subcutaneous operation devised by Professor Syme, and performed with such marked success by Mr. Whipple and myself, in the cases now reported. I do not relate them on account of their novelty, but I do so, to assist in bringing a safe operation into repute, and to rescue it from that oblivion under which it appears to be in danger of sinking, when we find that such an accomplished surgeon as Mr. Fergusson still prefers the ancient plan. Surgeons often regard an innovation upon the beaten paths of practice with suspicion or disfavour, and are content with the fair amount of success attached to any established operation. Thus valuable advancements in surgery become obsolete, or having fallen into disuse, are exhumed by later observers, and sometimes claimed as new discoveries. Look at the history of resection of the knee-joint, now an established operation—when first introduced by Park and the Moreaus, it was speedily lost sight of because its first efforts were not strikingly successful. A very few years since, this operation was revived by Mr. Fergusson, whose skill and love for conservative surgery have rarely been equalled. May not, therefore, this truly scientific application of subcutaneous surgery become obsolete, if its early success is not duly impressed upon the profession. Moreover, when a new and important operation based upon right principles has been introduced, it is surely incumbent upon its originator, as well as its first promoters, to chronicle all cases treated by it, in order that a proper estimate of its value may be entertained, and its claim to general adoption tested.

It is curious, perhaps instructive, to note, that all these cases of loose cartilage in the knee-joint, occurred in healthy countrymen, and in every instance but one, in those engaged in farm operations. All were young men, or at any rate, young when the disease was detected.

Beclard, Andral, Rokitansky, Syme, Adams, and other pathologists, have written and propounded their views upon the formation of these bodies.

Without any pretension to long and patient investigation, I would state my conviction, that a very large proportion of these loose bodies are the result of local injury. That by accidental violence, small or larger portions of the normal cartilaginous structure is detached. How many patients refer to some particular injury as the antecedent and cause of the disease.

It is pretty certain that in the 3rd case, that of Mitchell, a large cartilage was resolved by the ordinary friction of the joint into three smaller ones. Do these bodies grow? James Ellis (Case 1.) stated

positively to me that the one in his knee had done so. Indeed, as they possess independent vitality, there is no reason why they should not increase in size as well as be sustained in life. Their independent vitality does appear to me a very curious fact. Is it maintained by the chemico-vital phenomena of endosmose and exosmose? I have never subjected them to microscopic examination, but some of them are enveloped in a fine membrane. Their appearance is peculiar, waxy, irregular and as if worm eaten. They usually consist of bone and cartilage in indefinite proportions.—*London Med. Review, Oct.*, 1861, *p.* 163.

37.—*Excision of the Knee-Joint;—Recovery with a False Joint, and a Useful Limb.*—[A patient was brought into the operating theatre at King's College Hospital a short time since, on whom Mr. Fergusson had performed excision five years before.]

The excision was performed on March 1, 1856. The patella was not removed, as it was only superficially affected; it was scraped and then replaced. The disease was found to be very extensive, especially of the tibia; and after removing two slices from that bone, Mr. Fergusson removed also a quantity of dead bone by the scoop. The recovery was for a time rather sluggish, but ultimately she got quite well, and was able to attend to her household duties, suffering no inconvenience whatever; she could easily stand on the leg, and could run upstairs or jump off a chair as if she had had no disease and no operation. Up to within five months her health had been quite good. The great feature of interest in this case was, that there was considerable mobility of the joint, which remained to the present time, and which Mr. Fergusson demonstrated to those present. It was the first case Mr. Fergusson had seen in which this had happened. It put an end to "the factious opposition of persons who knew nothing on the subject, and yet declared that no good result could be obtained unless anchylosis occurred, and that this was so rare, that the operation must therefore be considered unadvisable." Since seeing the good result of this case, he (Mr. Fergusson) had been indifferent as to whether the result was osseous or fibrous union.—*Med. Times and Gazette, June* 8, 1861, *p.* 601.

38.—TREATMENT OF HOUSEMAID'S KNEE BY THE THREAD SUTURE.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

Mr. Skey's practice is to pass through the tumour a thickish thread, which is allowed to remain in. This sets up inflammatory action, known by a little redness around the entrance of the thread, and the swelling either subsides altogether, or what is more common, an

abscess forms, which is opened, and the cavity becomes obliterated. In these three patients this treatment was followed out, and suppuration took place in all, with the result of cure. In one—the girl of sixteen—erysipelas was contracted in the knee, and in the foot of the same leg, from a patient in the neighbouring bed. An abscess formed in the foot, which was opened, and the erysipelas is disappearing under the use of quinine internally.

Mr. Skey applies the seton to all forms of housemaid's knee. He thinks it is perhaps better suited, however, to the hard and indurated bursæ.

There are some examples of this disease, wherein the walls of the cyst have become so thick and solid that no plan of treatment short of actual removal will prove of any avail. We have seen Mr. Fergusson, at King's College Hospital, dissect them out, under such circumstances, with good results. And we can call to mind an instance that came under our notice some months back, at University College Hospital, under Mr. Erichsen's care, of a girl who had a bursal tumour of this character wholly removed.

In October last, Mr. Quain had a girl, aged 19 years, under his care in the same hospital, in whose left knee was a fluctuating bursal tumour, of the size of a small orange. This was treated by a thread seton, with the result of causing evacuation of its contents, mild suppurative inflammation, and obliteration. In that instance the tumour had been present ten months, and arose from kneeling while scrubbing.—*Lancet, April* 27, 1861, *p.* 409.

39.—EXCISION OF A PORTION OF THE OS CALCIS FOR CARIES.

By Dr. John K. Barton, Surgeon to the Adelaide Hospital, and University Lecturer on Practical Anatomy, Dublin.

Maria Rothwell, a little girl from the county Dublin, eleven years of age, of remarkably fair, clear complexion, light hair, and blue eyes, was admitted into the Adelaide Hospital upon the 5th of last February. Six months previously she had hurt her left foot, and soon after perceived a small swelling near the heel, which after some time burst, and continued to discharge purulent matter ever since. The pain and inability to use the foot was increasing. Upon examination, the left heel, measured from one malleolus to the other, was two inches larger than the right; it was a peculiar oval or rather globular shape. Upon the inner side, about half way between the malleolus and extremity of the heel, were two sinuses, through which, when the probe was introduced, the bone could be distinctly felt, quite bare, and breaking down when pressure was made upon it by the probe. The integuments upon the inner side of the heel were of a livid, purplish colour; just at first there was some constitutional

disturbance, but this soon disappeared when the heel was poulticed, and perfect rest was insisted on. The treatment adopted was both local and constitutional. The local consisted at first in the application of three leeches to the inner side of the heel, followed by a poultice and perfect rest of the part; this was succeeded by a diminution in the pain, and lessening of the swelling in the part; so that in about a fortnight the poultices were discontinued, and the heel was firmly strapped with soap plaster. The constitutional treatment consisted in a tablespoonful of cod-liver oil, with ten drops of the muriated tincture of iron, twice daily, and a milk and meat diet. This treatment had so far succeeded, that by the middle of March, her general health was greatly improved; the sinuses still remained open, but the discharge was very small, and there was very little pain in the foot. She was accordingly sent back to the country, with directions how to pursue the treatment, and not to use her left foot at all; and to present herself again in two or three weeks. Before the end of this time, however, she returned to the hospital much worse, having, as she confessed, used the foot as before, although cautioned not to do so. The whole of the posterior half of the foot was now much swollen. Through the sinuses, as before, the bone could be felt, but to a still greater extent. There was considerable constitutional disturbance. Perfect rest and poulticing reduced the inflammation round the diseased bone; but it soon became manifest that the caries was extending. An abscess formed beside the tendo-achillis, from which, when opened, a sinus was found to extend to the back of the calcis, so that the internal and posterior surfaces of the bone were engaged at least. Finding that rest, careful strapping, iodine locally, with iron and cod-liver oil internally, were not followed by the improvement now which they produced at first, and as there was every likelihood, that as the disease was not decreasing, it would soon engage the other bones of the tarsus, I determined to endeavour to save the foot by removing a part, or the whole of the os calcis. The latter alternative to be adopted, if, when the diseased part was laid bare, it became plain that the entire, or nearly the entire of the bone was engaged. From the position of the sinuses, and from the evidence given by the probe, however, I was led to hope that the diseased part of the bone was so situated that it could be sawn off from the rest.

Upon the 17th of April, the child was placed on the operating table, and a few drops of chloroform in a towel, in the usual manner, held before her face; she immediately, however, resisted this very much, and when, partly by persuasion, partly by a certain degree of force, she was induced to take one or two inspirations of the vapour, she immediately became pale, and her pulse fell alarmingly; it was, in consequence, immediately discontinued, and I proceeded without it. First, a semilunar incision about four inches in length, connecting

the sinuses upon the inner surface of the heel was made, laying bare the bone, which when the flaps were dissected up a little, could be felt very rough and crumbling, upon this its inner surface. The incision being prolonged backwards to the tendo-achillis, the bone was felt to be diseased above its insertion; it was, therefore, divided, and the whole cap of the heel dissected from the bone as quickly as possible, until the outside of the bone was reached, where there was no appearance of disease. Now the inner surface was again examined, and I found it necessary to dissect an anterior, or inner flap, from this surface up to the malleolus, the knife being kept very close to the bone, the posterior tibial artery being felt all the time beating in the flap, the back of the knife being towards it. When this much was accomplished, my colleagues who were present, agreed with me that a section of the calcis might be made, which would remove the diseased portion. A Butcher's saw was, consequently, placed upon the outside of the bone, and carried quickly in a direction upwards and inwards: by this the posterior half of the bone was removed. As soon as this was accomplished, I carefully examined the state of the remaining portion of the upper and inner part; it felt soft, so, with a strong gouge, I freely removed every suspicious part. The flaps were then simply laid together, supported by a few broad strips of plaster, wet lint laid over this, and the foot placed upon the outside. The child being removed to bed, got an opiate, and passed a quiet day.

An examination of the portion of the bone removed, showed the outer side quite hard and healthy, the inner soft and carious; while at the posterior part, just above the insertion of the tendo-achillis, there was a necrosed piece about the size of a large pea, surrounded by carious bone; this necrosed piece was hard, quite unlike the soft disintegrating bone round it, but was firmly united to the bone below it. The presence of this piece of dead bone seemed to augur well for the result of the operation, as this source of irritation had been completely removed. The little patient went on very favourably; the constitutional disturbance was trifling, but the wound only united partially by the first intention, and discharged an unhealthy serous pus for some time. It was kept firmly supported with strong adhesive plaster, and occasionally the granulating surface freely touched with sulphate of copper and nitrate of silver. In about a fortnight the line of incision was nearly healed, but a surface, about the size of half-a-crown, remained open at the back of the heel, while just above the malleolus a little abscess formed which was opened, but no diseased bone could be discerned here. The cap of the thick integument of the heel, which, immediately after the operation, seemed so much in the way and so loose, was now contracted firmly to the parts beneath, not appearing in the least too large, and making an excellent heel. The cicatrization of the parts which were granulating took place very slowly. This was attributable to the state of the consti-

tution, which was markedly strumous, but up to the present time, nearly three months since the operation, there has been no return of disease, either in the remainder of the calcis or in any other of the bones of the foot. The little girl only left the hospital on the 5th of this month, her foot presenting a very good shape and firm heel, upon which, however, she has not yet ventured to rest much of her weight, running about with the aid of a crutch or a stick. She can bear pressure upon any part of the heel or foot without the slightest pain. She has as yet but slight motion of the ankle-joint.

In this case we may infer, as far as the time elapsed since the operation permits us to judge of its effects, that the removal of a portion of the os calcis has saved the remainder of the foot. The disease was manifestly extending, and if so, must have necessitated finally the removal of the whole foot. Should the disease return at any future time, the patient will not be in a more unfavourable state for an amputation than she was at first. The result of the case so far, seems to me to encourage us in the attempt to give the patient, when youth and a tolerable constitution are on our side, a chance of preserving a limb, by the operation of excision of the carious bone.—*Dublin Hospital Gazette, July* 15, 1861, *p.* 215.

40.—*Pirogoff's Operation.*—Case under the care of Professor Fergusson, at King's College Hospital.—The patient, a boy, had suffered from diseased foot for some length of time. He was placed under the influence of chloroform. Mr. Fergusson made a section across the sole of foot, and sawed through the calcaneum from below upwards, previously to disarticulating the joint. Mr. Fergusson said he considered this a decided improvement in simplifying the operation. He then made a transverse section over the dorsum of the foot, and disarticulated the tarsus. Finding the internal malleolus of the tibia a little projecting, he sliced a small portion more off the calcaneum to receive it; he preferred doing so to removing the malleolus. He found the external malleolus required humouring in a similar manner. Mr. Fergusson said, sloughing was advanced as an objection to this operation: no doubt it does occur, but Mr. Fergusson did not consider it a valid objection. He said, Mr. Syme was the first to introduce removal of foot after this mode; previously amputation being always resorted to, and sometimes very high up. No doubt, Mr. Fergusson said, Pirogoff derived his idea from Syme's operation. Both Mr. Pirrie's and Mr. Watson's, of Glasgow, were modifications of this operation. We believe Mr. Hancock has performed a very clever mode of removal of foot.—*Med. Circular, July* 24, 1861, *p.* 64.

41.—ON DISLOCATIONS OF THE SHOULDER-JOINT.

By Dr. N. R. Smith, Professor of Surgery in the University of Maryland.

[Dr. Smith has for many years practised the following method of reducing dislocated shoulder, and has found it to be most efficient.]

Extension, counter-extension, and manipulation, constitute the mechanism of the operation of reduction. Counter-extension is the mere fixing, or rendering firm and immovable, the scapula. The head of the humerus alone requires to be relatively moved. The immobility of the scapula effected by counter-extension, is obviously of essential importance. All the movements of the head of the bone, effected in the effort to reduce the same, would merely drag or thrust the scapula loosely about, without changing the relative positions of the head and glenoid.

To effect the immobility of the scapula is the real difficulty in the reduction of this dislocation. In many of the methods practised, the bands employed for this purpose oppose the return of the head of the bone to its place. By some a slit is made in a broad band of stout muslin, the arm carried through it, and one margin of the slit brought into the axilla and the other against the acromion. The tails of the band are carried, one across the breast, the other across the back, horizontally, and are secured to the wall. The lower margin of the slit supports effectually the lower portion of the scapula, but the support of the upper, which is by far the most important, is not well effected. The margin of the band cannot be expected to remain opposed to the thin edge of the acromion. It will either glide over the top of it, or fall beneath it into the depression caused by the dislocation. In the former case it will not support the scapula above, but will allow it to be drawn outward and downward. In the latter case it will, by occupying the cavity to which the head of the bone is to be returned, hinder the reduction.

In effecting counter-extension, it is undoubtedly, in most instances, expedient to apply our resisting bands as directly as possible to the bone from which the other is dislocated. But the difficulty in this case is to effect it without defeating the object in the manner indicated above.

On noticing the mechanical relations of the scapulæ, with their apparatus of muscles, to each other, it occurred to me to make counter-extension from the opposite wrist, an expedient directly at variance with the commonly received principle, because as remotely as possible from the bone to be supported. There are exceptions to most rules, and I shall endeavour to establish this as one.

Let us observe the continuity of ligament, bone, and tendon by which the two scapulæ are bound together and made mechanically dependent upon each other. Anteriorly the two acromion processes are bound together by an unyielding chain of bone and ligament.

The two clavicles, the sternum, and the interclavicular ligament chiefly constitute this bond of union. The fibrous-resistent ligaments in this chain are not capable of being stretched. If traction be made from opposite wrists, the two acromion processes, thus tied together, are not capable of being drawn asunder to the extent of half an inch.

Posteriorly the continuity of resisting parts is almost as perfect. The broad expansions of the scapulæ approach each other not remotely, and they are bound to each other by the interposition of the ligamentum nuchæ, and indirectly by muscles and tendons attached to the spine. Traction from the two extremities will not therefore separate the scapulæ to any considerable extent.

The superior angles of the scapulæ indirectly support each other through the medium of the cervical portion of the spine. The levator muscles, arising from the superior angles of the scapulæ, pass upward to be inserted into the transverse processes.

Now the object of counter-extension is to prevent the yielding of the scapula to the tractive force exerted upon the dislocated member. Nothing does this so effectually as the fixing of the opposite scapula by counter-extending from the wrist. Not only is the scapula thus sustained, but the spine erected and prevented from yielding to the tractive force, and becoming curved to the injured side, as invariably happens when counter-extension is chiefly made from the axilla, as is usual. Let the experiment be made by allowing a person to incurvate the spine laterally, and then making traction horizontally from the two wrists. The spine will be immediately erected and the two scapulæ will at once assume symmetrical positions. And so in dislocation, the symmetry of the two sides tends at once to be restored. The head and spine are elevated, the two scapulæ firmly sustained, and effect is given, both to the tractive force, and to the necessary manipulation.

In some of the first cases in which I employed this method, I directed simply traction from the two wrists, and I am not now confident that this is not the best method. I placed the patient in a chair and directed two strong persons to make steady horizontal traction from the two wrists. As soon as the spasmodic resistance of the muscles was overcome, the head of the bone was disengaged, and the muscles which help us in such cases, suddenly lifted the head into its place. It will be observed that this method causes no appreciable pain, but rather relieves the suffering of the patient caused by the pressure of the head of the humerus.

It is now some twenty years since I commenced the use of this method; I am not, therefore, premature in expressing my confidence in it, and recommending it to the profession.

In cases in which, from unusual muscular development, or the age of the dislocation, much resistance is expected, I have modified the application of the counter-extension thus:—

I place the patient in a chair, sitting a little on one side of it, so

as to allow room on the side of the injury for the operator's foot. I then pass a piece of stout muslin, folded, around the chest and under the axilla of the injured side. The tails of it I carry horizontally to the opposite side, one in front, the other behind, and extending the arm horizontally, bandage them firmly to the wrist of the sound side, leaving the ends projecting, to be well secured to the wall, or other unyielding substance.

I then pass an ordinary roller over the top of the injured shoulder, and back and forth, twice under the muslin band, to prevent its slipping down. Then I continue the same roller under the bottom of the chair and over the shoulder, three or four times. This helps to give steadiness to the scapula, and especially to prevent the involuntary rising of the patient from the chair, or the tilting of the scapula upward, when it is necessary to make the manipulation of which I am to speak.

I now attach the extending band to the wrist of the injured side. I am aware that this is counter to the practice of many very eminent surgeons, especially Sir A. Cooper. My reasons are these: First, the wrist furnishes, as we may say, a very convenient handle to the arm. The band employed, if properly attached, does not slip, nor lacerate the integuments. In cases in which no great resistance is expected, it can be conveniently grasped by the hands only of one or two persons. In the next place, this method gives much more mechanical advantage in the way of leverage, in executing the final manipulations by which the head of the bone is thrown into place. It also removes the bands and hands of the assistants further from the patient's chest, and gives the surgeon more free access to the shoulder and more command of the arm.

The principal argument in favour of attaching the extending bands above the elbow is the relaxing, by this method, of the biceps muscle, which, by the complete extension of the forearm, is rendered tense. This disadvantage in my opinion is more than counterbalanced by the advantages mentioned above. The biceps is not capable of insuperable resistance; besides, a certain tension of its tendon, where bound to the head and neck of the bone, is favourable to the reduction, and it is partly the action of this muscle that finally, with a sudden shock, restores the bone to its place. Besides, when the bands are applied above the elbow, the forearm being at right angles, from the form of the arm, if any considerable traction is made, they will invariably slip over the elbow, make very painful pressure on the sensitive parts in the bend of the elbow, and not only cause great suffering, but extend the forearm and thus defeat the object in view, so that the advantage sought is lost, and many disadvantages encountered.

I first apply a wet roller to the wrist, and then attach a muslin band by the clove hitch. Next I direct the extension to be made by two persons, at first outward and a little downward, gradually raising the arm to the horizontal direction, and finally a little above it. The

extension must be made gently and steadily, gradually increasing the force, so as not to provoke the muscles to spasmodic resistance. As no pain is created by the force thus employed, it may be continued for a considerable time. The muscles, which at first resist, become fatigued and finally relaxed, and, in a large majority of instances of recent luxation, the head will slip into place without resort to any species of manipulation. I would even continue this traction, where much resistance is encountered, for a quarter of an hour before modifying the force; but, in case the object is not then effected, let the surgeon place his foot on the margin of the chair, and his knee in the axilla. Then let the assistants raise their line of traction above the horizontal as much as possible, and continue it for a moment. The surgeon should then direct that the arm be, by a sudden movement, carried downward, while, by extending his foot, he elevates the knee in the axilla. He aids the assistants in this by grasping the arm near the elbow and using it as a lever. If the first effort is not successful, repeat it.

Sometimes I place on the knee a ball made by rolling up a bandage, but it is not important. The surgeon can thrust his knee into the axilla so as to avoid much pressure on the marginal muscles of the axilla.

I generally direct those who make the traction to sway the limb horizontally backward and forward, and, grasping it with my hands, at times rotate it a little on its axis, thereby contributing to the disengagement of the head of the bone.

Where the consecutive displacement under the coracoid has occurred, the procedure is nearly the same, except that I make the traction a little more in the direction backward and upward, so as to disengage the head from under the process.

The dislocation upon the dorsum scapulæ, as I have before remarked, I have never seen but in three instances, and only one of those was a recent case justifying the attempt at reduction. It had been dislocated five days, and had resisted an attempt at reduction in the hands of others. The subject was a female, and I could obtain no very satisfactory account of the mechanical mode of the injury. It was easily recognized, there being a tumour beneath the spine of the scapula, as Sir A. Cooper describes, of the size of the hemisphere of a small orange, the limb being shortened and thrown forward. A depression existed under the anterior margin of the acromion. I failed to effect the reduction in the method usually recommended, and which I had deemed the best. I did not then practise extension from the opposite wrist. I made it by carrying the arm through a slit in a sheet. The extension was effected from the wrist, and when continued for some time, I sought to throw the head into place by manipulation—that is, I placed my knee against the back of the neck of the bone, and, swinging the arm backward, endeavoured to prize the head into its place. I repeated the effort several times, but with no satisfactory result.

I then carried a band over the front of the shoulder, one tail under the axilla, the other above it. These I united, carried them backward and inward obliquely, and secured them to the wall. Then I made traction strongly from the wrist almost directly forward. Without much difficulty I thus drew the head of the bone forward over the margin of the glenoid, and had the satisfaction to see it slip into its place.

Much has been said of late of the sufficiency of *manipulation*, to the exclusion of extension, in the treatment of dislocations. By employing new terms, and ignoring the precepts of old surgeons, claims to originality have been set up. Although the term is modern, the method of manipulation has been practised for centuries. It is, in my opinion, ridiculous to throw away the advantages derived from extension for the sake of magnifying the importance of a particular method of manipulation. Mothe, a long time ago, reduced the dislocation of the shoulder into the axilla by placing the patient in a supine position, and carrying the arm outward and finally upward, parallel with the head and neck, making traction at the same time.

It is well known that a recent dislocation into the axilla, may sometimes be reduced by placing the knee in the axilla, one hand being placed on the shoulder, and the other using the humerus as a lever.

The supine position of the patient on a table is a very convenient arrangement in all dislocations of the shoulder, and is well calculated to promote the efficiency of the method which I recommend. I am not sure that it is not the very best, especially when chloroform is employed.

It will be observed, then, that I advise the combination of traction and manipulation. It would be as absurd to reject traction in all cases of dislocation, as it would be to attempt the reduction, by manipulation alone, of the fractured femur, where overlapping and shortening had occurred.—*American Journal of Med. Science, July* 1861, *p.* 20.

42.—ON THE TREATMENT OF FRACTURE ON THE LOWER EXTREMITY OF THE RADIUS.

By Dr. Alexander Gordon, Professor of Surgery, Queen's College, Belfast, &c.

[The radius is perhaps broken more frequently than any other bone. The fracture usually occurs at or about an inch above the carpal end.]

The fracture is usually transverse, and both fragments—that is, the upper end of the lower fragment, and the lower end of the upper fragment—are displaced forwards and inwards. Of this double displacement, I regard the inward as of secondary importance, because,

if we counteract the forward displacement, the fragments, in resuming their natural position, will incline outwards; just in the same way as a straight or slightly curved piece of wood, when broken, with the fragments still in apposition, will resume its straight or slightly curved form when the deflection is counteracted. This fact, I think, has probably been too much overlooked hitherto in the treatment of this very common fracture; and hence too much importance has been attached to the necessity for counteracting the action of the pronator quadratus and other muscles by maintaining the hand in a state of adduction. Adduct the hand as we may, the deformity and its consequences will persist, if the forward displacement be not thoroughly rectified.

The practice hitherto adopted may be said briefly to have consisted of the application of a pad resting against the palmar surface of the forearm, extending as far as, or below, the lower end of the upper fragment and supported by a splint; of graduated compresses upon the posterior surface of the carpal fragment and carpus, and over these of a straight or pistol-shaped splint, and of adduction of the hand. Dupuytren applied the ordinary apparatus for fractures of the forearm, and relief chiefly upon an ulnar splint, to which the hand was held adducted, evidently overlooking the main displacement forwards.

Nelaton applies a compress, doubled at its lower end, so as to form a thick border, which he places immediately above the lower end of the upper fragment, and over that a splint, which extends beyond it, leaving between this splint and the lower end of the forearm a space which corresponds to the lower fragment; in fact, on the palmar surface of the lower fragment there is no pressure exerted. Posteriorly, however, he places across the posterior surface of the carpus and the carpal fragment of the radius two or three graduated compresses, of a wedge shape, with the base directed outwards, and over these a splint. Thus, according to Nelaton's plan, the lower end of the upper fragment is pushed backwards by the palmar compress and splint, whilst the carpus and carpal end of the lower fragment are pushed forwards, and inclined towards pronation.

A little reflection will show us, especially when we examine attentively the conformation of the forearm, when placed midway between pronation and supination, that any pad applied in front, and covered with a splint, will exercise its chief pressure along the centre of the forearm, and will not be sufficiently oblique to restore or maintain fully and effectually the concavity of the radius. The splint which I use has attached to its radial border a piece of wood, so bevelled that it fills up and fits accurately the natural concavity of the radius. The fracture renders the palmar surface of the radius convex, instead of being concave, its normal form. The surface of the attached piece of the splint is also convex, and covered with tow, a piece of spongio-piline (the best material), or some other

soft substance. The splint being thus covered, is then applied to the forearm. The convexities of the splint and radius are mutually brought into apposition; about half an inch, or perhaps a little more, of the lower end of the radius, is unsupported, because of the alteration in its form. A thick pad, similar to that used by Nelaton, is now applied over the posterior surface of the carpal fragment of the radius and carpus, and over this a splint, extending from the upper and back parts of the forearm to the metacarpus.

The action is very simple. The radius, at the seat of fracture, is convex, instead of being concave; the convexity of the splint is pressed against the convexity of the radius. The lower ends of the carpal fragment, the carpus and metacarpus being inclined unnaturally backwards, are pressed towards the palmar surface, acting as a lever, whose fulcrum is the convexity of the radius. The displacement forwards of the radius gradually yields; and if the pressure by the bandages be cautiously and continuously maintained, the convexity of the splint slowly and steadily forces the displaced fragments backwards, until at last the convexity of the splint rests in the concavity of the radius; and just as the inclination forwards of the fragments disappears, so does the displacements inwards; and so perfectly does the radius regain its normal form, that in most cases, it would be difficult to detect the seat of fracture. In one case, I tried in how short a time the splint would reduce the deformity under pressure from straps and buckles. The patient was a boy, 13 years of age, who "had sprained his wrist" nearly three weeks previously. The inclination forwards was even greater than usual. The splint was applied at ten o'clock a.m.; at 8 o'clock p.m. of the same day, the straps were tightened by one of the resident pupils of the General Hospital: the patient, in the mean time, made no complaint of pain; and on the following morning the radius had regained its shape perfectly. Yet I do not recommend that the correction of the deformity should be left to the splint and pressure alone. I merely mention it as a fact repeatedly observed, that the splint if properly applied, and aided by a tight bandage, will, as it were, mould the radius upon it, and restore its natural form.

A very important question arises at this stage of the inquiry. Can the methods of treatment recommended by Dupuytren, Nelaton, Dr. R. W. Smith, and others, restore the natural form of the radius, or can they maintain it when it has been restored by the manipulation, until firm osseous union has taken place? I believe they cannot, and for this simple reason, they do not afford a sufficiently solid mechanical support effectually to counteract the action of the supinator radii longus, the adductors of the thumb, and the other muscles, when there is a tendency to displacement; and if they do not, are those disastrous consequences which too frequently follow the accident to be referred to the injury inflicted at the time of its occurrence; or are they the result of inflammatory action subsequently

set up in consequence of alterations in the natural relation of the parts?

When the radius is restored to its natural form, and thus maintained, the "stimulus of imperfection" ceases, and, as in other fractures, very little serious inflammatory action follows. I have so often witnessed relief of the pain, which was very severe before the application of the splint, that I regard pain as indicative of displacement rather than of fracture. In some persons, especially those in whom the general health is not good at the time of the accident, the subsequent inflammatory action may be great, and the result of the primary injury. When once it has commenced, it may continue until it ends by impairing the functions of the limb permanently. Still my firm conviction at present is, that in most cases the inflammation is not of primary, but of secondary occurrence, and that it is evidently due to the non-restoration of the natural form of the radius. If we examine a case carefully by dissection, in which there has been a slight inclination forwards of the concavity of the radius, we shall see changes sufficient to account for it. Do not the same train of symptoms arise in cases of fracture of the fibula, in which, after the consolidation of the fracture, the weight of the body is thrown more inwards upon the tarsus than it was before the patient met with the accident?

I have before me six specimens of the radial fracture, and in none of them has the natural concavity of the radius been restored. Dr. R. W. Smith, in his admirable treatise on Dislocations and Fractures in the vicinity of Joints, says: "In not one of the twenty specimens which I have lately examined, has this object" (the restoration of the natural concavity of the radius) "been accomplished: in some it has been so far effected that the carpal surface looks directly downwards, but in none of them does the posterior surface exceed the anterior in length; in those which present the nearest approach to the natural form of the bone, these surfaces are of equal length."

Now, what are these alterations which result from the changed position which the carpal surface of the radius occupies?

The inclination forwards of the upper end of the carpal fragment causes its carpal surface to look downwards, and slightly backwards, which is also further increased by the greater absortion of its posterior half, evidently showing that the movements of the carpus upon the carpal surface of the radius are directed more towards its posterior border than natural; or in other words, the centre of motion between the carpus and carpal surface of the radius is displaced backwards. Indeed, whilst the length of the palmar surface of the lower or carpal fragment seems to be little, if at all, altered, the posterior surface is shortened one or two lines by the progressive absorption of the posterior half of the carpal surface. One of the effects, then, of this accident is to wear away, if I may use the expression, the posterior

half of the carpal surface, and cause shortening of the posterior surface, in some instances, by two lines at least; and leading to increased capability of extension, and diminished capability of flexion of the hand upon the forearm; also alteration in the line of action of the extensor muscles behind, of the flexors in front, and of all the other muscles which pass from the forearm to be inserted in the hand.

I have seen absorption of the vertical thickness of the scaphoid and semilunar bones. In one of the specimens of this accident which I have before me, the vertical thickness of the scaphoid and semilunar bones is unquestionably diminished; but as there is evidence of the presence of that morbid action to which the name of chronic rheumatic arthritis is given, the alteration may be as much the effect of this morbid process as of the accident. From the inclination inwards of the upper end of the lower fragment, the styloid process of the radius ascends, drawing with it the hand, and giving to the extremity the appearance as if the hand had suffered a slight partial dislocation outwards, causing projection of the lower end of the ulna, elongation of the internal lateral ligaments, and increased mobility of the carpal end of the ulna.

We may now perceive very plainly, from the changes in the natural relations of parts, and consequently in their actions, that the ligaments and sheaths of the tendons sustain a sort of chronic sprain, which, exciting inflammation, ultimately ends in greater or less impairment of the functions of the forearm and hand; that these occurrences are almost entirely referable to the displacement of the lower fragment of the radius; and that every mode of treatment which does not effectually restore the concavity of the radius must necessarily be followed by those distressing consequences which most surgeons have regarded as unavoidable and irremediable. That these distressing sequelæ may be almost entirely prevented, I have not the slightest doubt: but, if so, this can only be accomplished by a splint accurately fitting the concavity of the radius, and effectually maintaining it. The treatment of fracture of the lower end of the radius will then be as easy as that of any other fracture, and unattended with any serious consequences as regards the future usefulness of the limb. I have often been told by practitioners that they have efficiently treated fractures of the lower end of the radius by compresses in front and behind, covered by straight or pistol-shaped splints. To this I reply: That your patient has had a fracture of the lower end of the radius. I admit; that you have set the fracture, restoring the natural form of the bone. I also acknowledge; that compresses and splints have been applied is another fact; and also that the patient has regained the perfect use of the limb. All these facts I admit. But I cannot admit, from what I have seen of this accident, that compresses, supported by straight or pistol-shaped splints, are adequate to restore the concavity of the radius and maintain its natural form, if there be a tendency to displacement,

because they are not adequate to the correction of the displacements; and, being mechanically imperfect, deformity of the radius and its disastrous consequences must result.

Every practitioner who has carefully observed cases of this accident, and treated them according to the most approved methods hitherto adopted, must, if possessed of ordinary candour, admit the existence of the serious consequences which too frequently attend it, and which have been faithfully described by Dr. R. W. Smith :—"The sequela of this injury is a source of great inconvenience to the patient, and of annoyance to the surgeon, who is often unjustly blamed for its occurrence; the practitioner will therefore act with prudence in warning his patient, at the commencement of the attendance, that stiffness of the wrist-joint, and an incapability of flexing the fingers, during a period of several months, are by no means unfrequent results of a fracture of the lower end of the radius." (p. 139.) It would be presuming too much to affirm at present, that these untoward circumstances can be entirely prevented. Still, I am bound to state, from considerable experience, that, under proper treatment, they are very rare; so rare, indeed, that their occurrence neither need be anticipated nor dreaded.

In the formation of the splint, there are several points which should be carefully observed. The breadth of its lower or carpal end should not exceed that of the forearm at the wrist; it should not project so much internally as to be on a level with the inner border of the forearm. On the radial side, it should project beyond the radius; and the bevelled piece, for filling up the concavity of the radius, should be attached half an inch, at least, internal to that border. By this arrangement, the ulnar side of the forearm sustains the pressure of the bandage or straps. The reverse is the case on the radial side. The radial border of the splint alone is pressed upon; this pressure forces the bevelled portion inwards upon the concavity of the radius and pushes it backwards, whilst, at the same time, the outer border of the radius is protected from pressure. If the ulnar margin of the splint projected beyond the ulnar border of the forearm, it would be pushed outwards when we applied the bandage; and the bevelled portion would be removed from the concavity of the radius, and it would be left unsupported. The upper end of the splint should be well hollowed to adapt itself accurately to the convexity of the upper part of the palmar surface of the forearm.

In this, as in other fractures, I prefer broad straps, with buckles, to the ordinary calico or linen bandage, as they are more easily removed, and less liable to loosen. Indeed, for some time past, in the treatment of fractures generally, I have entirely discarded the ordinary circular bandages, using in their stead broad straps, with buckles attached; and whether we regard the comfort of the patient, or the satisfaction which the straps afford to the surgeon, the advantages

are so obvious, that calico or linen bandages cannot bear comparison with them.

Another point to which my attention has been particularly directed for some time, has especial reference to the diagnosis of injuries of the wrist, and ankle-joints, and may be of some importance to the junior practitioner. Fractures of the lower end of the radius are usually caused by falling upon the upper part of the palmar surface of the hand, when the patient instinctively extends his arms, whilst falling, to prevent his face coming in contact with the ground. If, after falling in this manner, a patient complains of having sprained his wrist severely, and is immediately afterwards unable to move it, unless with great pain, it is the radius and not the wrist-joint that has suffered. I cannot bring to my recollection a single instance of severe pain caused in this manner. I do not mean to deny the possibility or probability of the wrist being sprained in this way; but I wish to impress upon the junior practitioner the importance of a maxim which I have laid down for my own guidance, and it is this:—When a patient comes to me and says, "I have sprained my wrist, and am unable to use my hand," I assume the injury sustained to be a fracture of the radius and not a sprain, until I have satisfied myself to the contrary. When a patient requires my assistance for a severe sprain of the ankle-joint, caused by the foot being twisted either inwards or outwards, I assume that he has fractured the fibula, and that the whole injury is a fracture and not a sprain, until, by the most careful examination, I have satisfied myself to the contrary. Sprain of the wrist, from falls upon the palm of the hand, is a very rare accident. I do not remember having met with a single instance of it. Sprains of the ankle-joint, from twisting of the foot, I also regard as very rare accidents; whereas fractures of the radius and fibula are very common.—*Edinb. Med. Journal, May* 1861, *p.* 999.

43.—COMPLETE RESTORATION OF THE TIBIA BY THE PERIOSTEUM, AFTER ITS REMOVAL.

By M. Maisonneuve.

In the month of August, 1855, M. Maisonneuve was consulted for a young man, whose right leg was in a frightful condition. Its size was enormous, and deep ulceration was present over its entire surface, and it was but too obvious that the whole shaft of the tibia was in a state of necrosis. The patient stated that some two years before, while engaged in gymnastic exercises, he had met with a severe fall, that deep-seated aching pain in the leg had been the consequence, together with general tumefaction of the limb, and subsequently abscesses, which had gradually brought about the present condition—viz., copious and fœtid suppuration, enormous increase of size of the leg, extreme emaciation, hectic fever, &c. The parents, moreover,

declared that all the resources of medicine had been exhausted, that several of the most eminent surgeons of Paris had unanimously recommended amputation, and that M. Velpeau, to whom, as a last resort, the matter was referred, had not only informed the family that the operation was indispensable, but further, that it was urgent, and that all hope of preserving the limb must be abandoned.

Despite these unfavourable opinions, emanating from such high authority, M. Maisonneuve, trusting in the regenerative power of the periosteum, proposed to extirpate the bone beneath that membrane, an operation which was consented to, and in M. Maisonneuve's words, was performed as follows:—"The patient having been fully placed under the influence of chloroform, I performed along the entire length of the tibia, in an extent of about fourteen inches, a section penetrating to the bone through the periosteum, which was already thickened, and lined with a new, soft, and spongy osseous secretion. At each extremity of this long incision, I divided the parts transversely, so as to be able, by turning over the flaps on each side, fully to expose the seat of disease. I thus ascertained that the entire shaft of the tibia was mortified, the articular extremities alone remaining healthy. I then proceeded, without delay, to separate the dead bone from the adjacent structures, and succeeded, not without difficulty, in extricating it completely. The consequences of this long and arduous operation were of a truly remarkable simplicity. The traumatic fever was extremely moderate; the suppuration, hitherto copious and fœtid, was speedily replaced by a healthy puriform secretion, and, extraordinary as it may appear, the young patient was able, so early as the fortieth day, to rise and walk with crutches, as if he had merely been affected with simple fracture of the limb. An entirely new bone had been formed, and so complete was the restoration, that had I not preserved the shaft of the tibia, removed six weeks before, I myself could scarcely have credited the fact."

This bone, which M. Maisonneuve placed before the Academy, is twelve inches in length, fifteen lines in thickness at its upper extremity, and eleven lines in diameter at its lower end. The three surfaces are smooth and compact in the two lower thirds, and rugged and swollen in the remainder. The patient is now healthy and vigorous; the formerly diseased leg differs in no respect from the other, except by the scar, which is now the only memento of the formidable process above described; he can run, leap, or exercise as if he had undergone no surgical operation whatever, and the most careful inspection fails in detecting which leg was the seat of disease. This remarkable instance of restoration of bone suggests the query, how many patients may be found who will survive the operation?—*Journal de Med.; and Dublin Hospital Gazette, June* 1, 1861, *p.* 174.

44.—ON ANTERIOR AND POSTERIOR CURVATURES OF THE SPINE.

By William Adams, Esq. Surgeon to the Royal Orthopœdic and the Great Northern Hospitals, &c.

These two classes of cases, viz., diseases and distortions, or deformities of the spine, were by the older Surgical authorities frequently considered as one and the same form of disease, but the wide separation between them, and their easy diagnosis—except, perhaps, in one form, viz., rotation of the lumbar vertebræ accompanied with severe pain, to which I shall hereafter especially allude—are too well known to render any observations necessary from me. I would, however, remark that in modern times, the distinction between these two classes of cases, viz., diseases and distortions of the spine, has been drawn rather too broadly, inasmuch as it has long been customary for surgical authorities to describe the distortions or deformities of the spine as merely functional affections, arising from muscular debility, &c., and unaccompanied with structural changes except in severe cases of long standing—or what may be called the later stages of these affections—whereas I shall endeavour to prove to you that it is anatomically impossible for any distortion of the spine, such as lateral curvature, however slight, to exist without being accompanied with structural alterations proportionate to the extent and duration of the curvature; and when these structural alterations are considered in connexion with the constitutional conditions upon which they essentially depend in a large proportion of cases, though their immediate causes may be of a mechanical origin, such as the irregular distribution of weight, &c., it cannot be doubted that such affections may be ranked among the simpler forms of disease, with more reason than they can be placed in the same category with the simple contractions and deformities which take place at the moveable articulations of the extremities from debility, position, and other causes indirectly affecting the articulations; and in which we know the joint surfaces preserve their integrity in form and structure, though they may deviate in their relative position to each other. I make these observations because, whilst I fully concur in the propriety of preserving for practical purposes, the broad lines of distinction between these different affections, it is important pathologically that we should determine where such broad lines shade off into each other.

The spinal column, then, is liable to *distortion*, *i. e.* fixed deviation from the erect position, in various directions so as to produce external deformity. The distortion may take place in the *antero-posterior* direction, either as an exaggeration of the natural curves of the spine which become so much increased as to produce external deformity; or the natural curvatures of the spine in the antero-posterior direction may be reversed, especially in the dorsal and cervical regions, *i. e.* the

dorsal curve may become depressed so as to project anteriorly, curving forwards instead of backwards; the cervical curve may project backwards instead of forwards; and the lumbar curve frequently becomes straight instead of curving anteriorly. Or distortion may occur as a lateral deviation, though I shall presently explain to you that the deformity described as *lateral curvature of the spine* does not depend upon a true lateral deviation, but rather upon a peculiar twisted, or contorted, condition of the spinal column, in which an external lateral deviation of the spinous processes generally, but not invariably, forms a conspicuous feature. In this affection, however, the bodies of the vertebræ always deviate laterally, even when the apices of the spinous processes—upon the external examination of which surgeons generally, but erroneously, depend as a test whether the spine is straight or not—remain unaltered in their relative position in the median line of the body. So far, then, as it relates to the altered position of the bodies of the vertebræ, the term lateral curvature is sufficiently correct for practical purposes, and the lateral direction of the deformity is rendered still more apparent by its effect in producing a prominence of one shoulder, or of one hip as it may occur in the dorsal or the lumbar regions, or of both shoulder and hip when it exists as a double curvature. I would therefore propose to continue the use of the term *lateral curvature of the spine*, because it carries with it the idea of a lateral *distortion*, as distinguished from the sharp posterior, or *angular curvature* produced by destructive disease of the spine.

Before describing the ordinary form of *lateral curvature* of the spine, I would observe that deviations of the spine either in the antero-posterior, or the lateral directions, may co-exist with destructive disease of the spine, affecting the intervertebral cartilages and the bones, more especially in the early stage; or they may be consecutive to such diseases, and remain as permanent distortions after the destructive disease has been completely arrested. For example, when destructive disease occurs in the dorsal region, as we frequently see it in children, and sometimes in adults, the posterior convexity of the spine is very much increased, and there may be great difficulty in deciding upon the existence or non-existence of disease. At a later stage, however, a sharp, angular projection of one or more spinous processes takes place, and all doubt as to the existence of disease is removed; but occasionally it happens that several intervertebral cartilages are affected without caries of the bones, as exhibited in preparations in St. Thomas's Museum, No. E 20 and E 22, and in such cases no angular projection of the spinous processes would take place.

When destructive disease in the lower dorsal and upper lumbar vertebræ has become arrested with the ordinary result of angular projection backwards of three or four spinous processes, the spine becomes altered in form, as a consecutive condition, in the middle dorsal region which is either depressed so as to form a straight line, or

the natural direction of its curve may be reversed, and a concavity instead of a convexity may exist in the middle dorsal region.

Then, again, when destructive disease attacks the middle and lower lumbar vertebræ, it occasionally happens that the natural concavity in this region is considerably increased in the early stage of the affection. I saw a very remarkable instance of this kind in a young woman, who was admitted into the Orthopœdic Hospital under my late friend and colleague Mr. Lonsdale. The increase of the lumbar curve and hollowness of the loins was almost as considerable as we see it in the cases of anchylosis of the hip-joint at right angles after disease, or in cases of congenital dislocation of both hips; the spinal muscles on either side were very prominent and tense, and a deep depression in the median line existed between them. The nature of the case was obscure; destructive disease was not suspected, the usual symptoms being absent; and it was regarded as essentially a spasmodic or spastic muscular affection, and the propriety of dividing the muscles discussed. Mr. Lonsdale, however, applied a spinal instrument with crutches and a webbing band in front. In the course of a few months the increased lumbar curvature disappeared, and afterwards a posterior, or rather angular, projection of the spinous processes of the twelfth dorsal and the first and second lumbar vertebræ appeared. The disease ran a remarkably favourable course, the more severe symptoms usually met with when disease exists in this region never appearing, and the girl has remained in service; she still continues to wear a spinal support, and comes occasionally to the hospital. Casts from this case are preserved in the museum of the hospital.

Deviations of the spine in a lateral direction may also occur in the early stage of destructive disease, when it attacks the bodies of the vertebræ, or the intervertebral cartilages, laterally instead of their anterior aspects as usual; or in the later stages, it may exist as a consecutive curve, either above or below the angular projection, resulting from destructive disease in the lower cervical, or the lower dorsal and lumbar vertebræ.

Curvature of the spine, then, either in the anterior, posterior, or lateral direction, may co-exist with destructive disease of the spine in its early stage without any reliable symptoms or indications of such disease being present. It is as well you should bear this in mind, because, in the course of practice, cases will occasionally present themselves in which the diagnosis must be one of considerable difficulty, as well as of great importance, and we know that disease of the spine in the early stage is at all times difficult of diagnosis; but, in the present lectures, it is not my intention to do more than advert to this class of distortions.

Curvatures or distortions of the spinal column, not connected with or dependent upon destructive disease of the bones or intervertebral cartilages, generally depend either upon muscular debility associated with some unfavourable constitutional condition, or upon the long con-

tinuance of some faulty position, the result either of acquired habit, or of some occupation; or they may depend upon both these causes combined. This is essentially the class of cases which I now propose to bring under your notice, and will first offer a few observations on the antero-posterior forms of curvature, before describing the lateral deviation.

Anterior Curvature of the Spine —Lordosis, λόρδωσις *from* λορδόω, *to bend supinely, so as to throw the head back.*—Curvature of the spine in an anterior direction, with the convexity in front occurs chiefly in the lumbar region, as an increase of the natural curvature, from several causes; but it may also exist in the dorsal region to a less extent, and more rarely it occurs in the cervical region.

In the lumbar region this increase of anterior curvature is found—

1st. As a peculiarity in the natural conformation of the individual —often existing in several members of the same family, and hereditary. It is more common in some countries than in others, but in England I have most frequently observed it in short people, with long bodies and short legs, *i.e.* belonging to the rachitic type of development, and whose bony development exhibits some indications of a rachitic tendency.

2nd. As a constant effect of rickets in its more severe forms, in which the entire skeleton indicates the existence of this affection in the stunted growth of the individual from arrested development of the bones—more conspicuous in the leg bones from their normal rapid rate of growth during early childhood as compared with the spine and other bones of the body, hence the disproportionate shortness of the legs in these cases—and in various curvatures especially affecting the leg-bones and the pelvis. In severe rickets, I need hardly explain to you that the pelvis becomes flattened from above downwards, from the inability of its bones to support the superincumbent weight, and the sacrum assumes a horizontal direction. As a necessary result of of this alteration in the direction of the sacrum, and the deviation in the axis of the pelvic cavity, an increase of the lumbar curve of the spine takes place to preserve the equilibrium of the body, and in these cases of rickets a very marked *lordosis* is the result.

3rd. As a constant effect of congenital dislocation, either of one or both hip-joints, an affection which occurs more frequently than is generally supposed. About twelve cases, or probably more, have come under my notice since this affection was first pointed out to me by Dr. Carnochan, of New York, in the year 1845, when that gentleman, being in England, brought a case of congenital dislocation of both hips, in a boy, aged 18, to St. Thomas's Hospital, for the purpose of demonstrating this affection, to which he had paid much attention. The external characters of the dislocation, and its effects in altering the conformation of the chest and abdomen, in consequence of the tilting forwards of the pelvis, and the production of lordosis to an extreme degree in the lumbar region, were so strikingly illustrated in

this case, that under the direction of Mr. South, an entire model of the boy was made by Mr. Kierney, and is now in the museum of the hospital.

This form of dislocation, in which the heads of the thigh bones are generally displaced upwards on the dorsum ilii, or upwards and backwards, appears to be very seldom diagnosed. Commonly no attention is directed to the condition of the hip-joints till some time after the period of walking, and then, when only one joint is affected, the limping of the child induces the surgeon to suspect the existence of hip-joint disease, and the case is treated accordingly. This error is diagnosis has occurred in several instances which have fallen under my observation in private practice.

When dislocation of both hips exists, the real nature of the affection is also generally overlooked, and I have known it said that the child would grow out of the peculiar waddling gait which characterises the walk in these cases. It is, then, in congenital dislocation of both hips that extreme incurvation of the lumbar region takes place, as a compensative effort to preserve the equilibrium of the body, disturbed by the tilting of the pelvis which necessarily results from the abnormal position of the heads of the thigh bones.

4th. Either co-existing with destructive disease of the lumbar vertebræ in the early stage, as in the case already described, or as a consecutive curve after destructive disease had become arrested, and angular curvature, *i.e.*, posterior projection of one or more spinous processes, produced in the middle or lower dorsal regions. In such cases the lordosis in the lumbar region is necessary to preserve the equilibrium of the body, and is, of course, persistent through life.

5th. As a constant effect of anchylosis of the hip-joint, with the thigh in a flexed position. After strumous disease of the hip-joint, rheumatic inflammation, and several other affections, anchylosis of the hip-joint, either ligamentous or bony, frequently takes place, with the femur in a flexed position, in most cases at an angle of about forty-five degrees, but sometimes at a right angle with the pelvis. The angle is generally much greater than is apparent at first sight, because in standing or walking, the patient brings the foot to the ground, by increasing the natural curvature of the spine in the loins, and in these positions, therefore, lordosis is produced, but it is more or less completely removeable when the patient sits down. In such cases we have a greatly increased amount of flexibility in the lumbar region.

Treatment.—Surgically speaking, *lordosis* occurring in the lumbar region is of very little importance, as in its worst forms it is only a part of a more important affection, and very seldom becomes the subject of treatment; but it may sometimes assist us in the difficult diagnosis of destructive disease occurring in the lumbar vertebræ, as it might have done in the case above described, had we known of the occasional co-existence of these affections, and sometimes, though very rarely, it may become the subject of special treatment. In con-

nexion, however, with the treatment of the diseases, such as rickets and caries which sometimes produce this form of curvature, I would especially direct your attention to a few practical points.

In the first class above described, viz., that dependent upon natural configuration or peculiarity, either of family or race, of course there could be no attempt at surgical interference.

In the second class—the rachitic form—no special treatment is indicated, but with the view of preventing this, as well as the more important pelvic distortion which accompanies it, I recommend as a general principle in cases of rickets, that the children be allowed to stand or sit as short a time as possible, *i.e.*, that standing or walking for any length of time must not be allowed, and that reclining or complete lying down be encouraged. It is a very serious, though common, practical error to send children in whom rickety curvatures have commenced, into the country, or to the sea-side, and encourage them to run about all day long; such children generally return with improved health, but with the curvatures more confirmed. This is very good treatment for slight cases of knock-knees and bow-legs, which children certainly do grow out of when the general health and strength are improved, but a great mistake in cases of true rickety curvatures, which we know are immediately produced by mechanical causes acting upon imperfectly nourished bones. I need not allude to the influence of muscular action, either in causing or increasing rachitic curvatures. The bones of the legs and the pelvis become distorted simply by the weight of the body, which they are unable to sustain, and, therefore, I remove the weight of the body as much as possible; substitute exercise in the horizontal for exercise in the erect position, and rely upon constitutional treatment, especially cod-liver oil with steel, and the hypophosphite of lime, &c., for promoting the cure of the disease, if so it may be called.

Children may be induced to take a great deal of exercise, as amusement, by pulling themselves up and down an inclined plane with ropes, pulleys, and weights, properly attached. They should be kept in the open air, in the country, or at the sea-side, as much as possible, and the reclining position in a properly-arranged carriage be encouraged. In many cases it may be impossible to carry this plan out as fully as could be desired, but if the principle be admitted, it may in all cases be adopted to a greater or less extent. We know that rickets, as a general rule, undergoes spontaneous cure with the increasing age of the child, and the alteration of its diet; and that the bones become even stronger than healthy ones, but that the deformities remain persistent through life. It may be said that rickety children grow out of the disease, but they grow into the deformity. The danger in females of the pelvic distortion, of which the spinal curvature or lordosis is the least important consequence, I need hardly advert to, but I

believe that all the rachitic distortions may be, to a great extent, prevented by the treatment I have mentioned.

In the third class, i.e., lordosis in cases of congenital dislocation of the hip-joint, it is very doubtful whether any permanent diminution of the spinal curvature can be effected by mechanical means, though I have seen it attempted, and with some appearance of temporary benefit. Possibly the lordosis may be diminished by the constant use of a spinal instrument from early childhood to the completion of growth, but as the hip-joint articulations must remain in their abnormal condition, it is very doubtful whether any permanent advantage would be obtained by such means.

In the fourth class, i.e., lordosis existing during the progress of destructive disease in the lumbar region, or as a consecutive curve after destructive disease has subsided and angular curvature been produced in the middle or lower dorsal regions—there can be no doubt of the very great advantage of mechanical support during the progress of caries or destructive disease in any region of the spine, and the case of lordosis co-existing with caries in its early stage in the lumbar region, previously mentioned, affords a good illustration of the advantage of this plan of treatment. In all surgical works, without any exception so far as I have noticed, the necessity of the patient lying down during the progress of caries or destructive disease of the spine, is strongly insisted upon ; but since my connection with the Orthopœdic Hospital, now nearly eleven years, I have invariably opposed this rule, and adopted the practice of applying mechanical support to the spine in these cases, by means either of a leather or steel apparatus, according to the age, and allowing the patient to walk about when so disposed. This treatment I find to be equally applicable to all stages of caries or destructive disease of the spine, from its commencement, which we may in most cases diagnose before any angular projection has taken place, and even when the destructive process is evidently advancing, provided the patient be able to bear the support, and is disposed to walk. The mechanical support enables such patients to walk about with comfort, and does not in any way interfere with, but I believe essentially promotes, the proper constitutional treatment of the disease. During periods of increased pain such as may occur in the stage of abscess, confinement to the bed is dictated by the patient's feelings, but as soon as the child (most of these cases are seen in children) is disposed to walk again, I allow him to do so. I never employ any local counter-irritation. All abscesses should be opened early by a valvular or subcutaneous opening, which I think is much better than allowing them to attain a large size, and the patient to suffer from the tedious process of pointing and bursting. In old cases of lordosis in the lumbar region, or consecutive to caries and angular curvature in the dorsal region, no advantage is to be derived from treatment, the curvature being a natural compensating effort to maintain the equilibrium of the body.

In the fifth class, i.e., lordosis consecutive to hip-joint disease, the curvature is also a natural compensating effort to maintain the equilibrium of the body, and there are no indications for surgical interference.—*Med. Times and Gazette, Oct.* 19, 1861, *p.* 395.

ORGANS OF CIRCULATION.

45.—TRAUMATIC ANEURISM OF THE FEMORAL ARTERY.

Case under the care of Dr. Thomas J. Rawson, in Carlow County Hospital.

[This case was treated first in the hospital, ten years ago. The circumstances under which the accident occurred, are related.]

David Lowe, (age at that time 49) the subject of the present report is a millwright by trade; he was sitting one morning in July, 1852, at a fire in the parlour of a public-house in Carlow, smoking calmly and watching with a philosophical eye the flies as they ran up and down the chimney-piece. He had been filling his pipe, and when he cut the tobacco he put the knife, "an ordinary clasp one," up on the chimney-piece with the blade open, but unfortunately for himself he put it up carelessly, and after oscillating on the edge of the slab for a moment down it came. Mr. Lowe was of a naturally active and energetic temperament, and as the knife was passing in its downward course between his legs, he brought them suddenly together with a jerk in the hope of catching it, and he succeeded in effecting his object perfectly, and something more besides, for he not only caught the knife, but he caught it at the moment its handle and point were fair across between his two thighs, and in bringing them together he drove it right into his left thigh under the sartorius, and wounded the femoral artery in Hunter's canal, of which immediate evidence was given by profuse hemorrhage; he called for assistance lustily, and Dr. Connor, being the nearest, was soon on the spot, but when he arrived finding there was a sheet torn into bandages and wrapped round it and all hemorrhage appearing to have ceased, he judged it better to leave as it was, and so it remained until, from his own account, the external wound was quite healed, which took place in two or three days, but it left behind it a large tumour, pulsating vehemently, and he was accordingly removed to the infirmary.

When Dr. Rawson first saw him, there could be no doubt, there was a traumatic aneurism of the femoral artery, the tumour covering three-fourths of the middle third of the thigh on its inner aspect. He, therefore, applied Carte's compress, and after a weeks' use the tumour began to decrease visibly, and the pulsation in it to lessen very much; it was, therefore, continued, and he was gradually getting better, when his unfortunate active and energetic temperament step-

ping in, he got weary of the compression and restraint, and left the infirmary directly against the wishes of Dr. Rawson; the tumour, however, at this time was not more than six inches round the base, and the pulsation was very faint indeed, so he seems to have gone about his ordinary occupations, and to use his own expression, did what he liked for a couple of months, but at the end of that time, the tumour being on the increase and the pulsation with pain and uneasiness returning, he applied again at the infirmary. Dr. Rawson then advised him to go to Dublin and see what could be done for him there; he went to the Richmond Hospital, and to his surprise and disappointment the compress was again applied; he said he might not have come all the way from Carlow to get that done, so not believing he was getting better, he came home and applied a third time at the infirmary. Dr. Rawson now determined to tie the artery, but when the day of operation came, Mr. Lowe's courage failed him, and he would not permit it to be done; the compress was, therefore, applied again, and with such success, that in about three weeks the tumour became reduced to the size of a very small hen egg, while the pulsations in it had almost entirely ceased; he could now walk about with the greatest ease, so he left the infirmary and he returned to his avocations, pursuing them with perfect comfort to himself for nearly ten years, until one evening in June last, his unfortunate active energetic temperament got the better of him once more, and he got down into some water to work at a wheel that was out of order, and exerted himself very much, which he needn't have done, and when he came out he found his foot quite numb and cold, and he couldn't warm it. He had no whiskey in the house, or he says the sequel would have been different, so he got into bed, and having suffered dreadfully the whole night, he came in the morning to the infirmary. On his admission, his leg showed deficient circulation and warmth, while the foot exhibited all the signs of incipient gangrene. He was very weak and low; the whole limb was wrapped in cotton and camphorated spirits, and he got wine and bark, opium and generous diet, under which treatment his leg gradually improved; the foot, however, getting worse, till on the fourth or fifth day sphacelus was too apparent at the toes, it spread up pretty quickly, and in a few days *the line of demarcation* was fully established at the ankle-joint, and the foot rapidly separated; the tendons were snipped through, and at last Dr. Rawson removed the foot, there being trifling hemorrhage from two small arteries which speedily yielded to cold.

He is now progressing most favourably; his appetite is very much better, and his tongue and pulse both greatly improved, which improvement is, in a great measure, due to himself; he is invariably cheerful, even while suffering extreme pain.

In trying to account for the recent gangrene, Dr. Rawson suggests that a portion of fibrine being detached from the aneurismal clot, by the blood which was circulating with very much increased rapidity

from the exertion he was making, was carried down to the popliteal and being unable to pass its bifurcation plugged it, allowing barely sufficient to supply the leg, but not enough for the foot, that after some days the collateral circulation enlarging in the leg it recovered; but the foot, at that time, was too far gone to profit by this resource of nature, and so unavoidably perished.—*Dublin Hospital Gazette, Aug.* 1, 1861, *p.* 229.

46.—*On the Treatment of Popliteal Aneurism by Compression.*—[In most instances in which compression is used, the patients complain of the pain they suffer, and the length of time necessary to effect a cure. With regard to these objections, however, Mr. Erichsen observes, that the amount of pain depends very greatly upon the skill and care with which the apparatus is applied and managed, as well as upon the instrument used. (Mr. Erichsen uses Carte's elastic instrument). He further shows, that, in point of time, the cure is about the same, whether from the ligature or compression.]

Professor Pirrie, who has taken the greatest interest in the subject of the compression treatment in aneurism, and who purposely visited Dublin to satisfy himself by personal observation of the advantage of "the bloodless cure of aneurism," says, in his 'Principles of Surgery': —"The result to my mind was, the firm belief that the treatment by compression is a safe, simple, successful, and *almost painless* mode of curing aneurism."

The great success which the Dublin surgeons have met with in this mode of treatment, accomplished too without the suffering of much pain, would show that there is something in the minutiæ of the application of the compressing force which is not as yet quite understood here. Either the pressure on the arterial trunk is too great, and hence the inability of the patient to bear it; or else the apparatus is too complicated for the due regulation of pressure in accordance with the feelings of the patient.

Statistics have proved that the risk to the patient from the use of the ligature is greater than that from compression.—*Lancet, May* 25, 1861, *p.* 509.

47.—LARGE GLUTEAL ANEURISM; LIGATURE OF THE GLUTEAL ARTERY.

Case under the care of Prof. Syme, at the Edinburgh Royal Infirmary,

In the 'Lancet' for June 8th was noticed a case in which Mr. Syme tied the internal iliac artery for a spontaneous gluteal aneurism. The ligature separated on the twenty-fifth day after the operation, and the patient continues to do well in every way. This will make the seventh case in which the internal iliac has been tied, in most of the cases with success.

By a singular coincidence a second case of gluteal aneurism has occurred at the Edinburgh Royal Infirmary, also under the care of Mr. Syme. There was a large aneurism of the left hip, of traumatic origin. Seven years ago the patient, a middle-aged man, received a wound of the hip from a pruning-knife accidentally driven deeply in, which was followed by great bleeding. The wound closed, but an aneurism formed, which latterly had become greatly enlarged. It was as large as a man's head at the base, occupying the whole hip, and rose into a large blunt cone. Friday, the 14th, was a field-day at the Surgical Hospital, the large theatre being crowded by students and members of the profession, assembled to witness a bold operation which has not been seen for sixty years, since the intrepid John Bell performed it in a similar case in the same hospital. The description which John Bell has given of his case has been regarded as exaggerated or coloured; but no one will think so who witnessed Mr. Syme's operation last week. The only respect in which Mr. Bell's narrative transcends is in regard to the length of his incision, which was two feet in length; but the tumour he described as of "prodigious" size.

Mr. Syme commenced the operation by plunging a bistoury into the tumour. This was followed by a gush of arterial blood, which spouted to a considerable distance, but was immediately arrested by the operator introducing the forefinger of his left hand, the wound having been calculated for this. With his finger in the sac, Mr. Syme now explored the interior, hoping to discover the situation of the artery by the entering current. Nothing, however, could be felt but a confused mass of clots. The operator then enlarged the wound cautiously until the whole hand could be introduced, which was now thrust in up to the wrist. As the hand was being used to search the interior of the sac, it was so held that the wrist was nearly sufficient to occupy the wound; but during this procedure several alarming gushes of blood took place, bespattering the operator and the bystanders, and most appalling to the onlookers, although in no way discomposing Mr. Syme. Compression of the abdominal aorta was tried, but as the patient lay nearly over on his face, this could only be done by pressing the abdomen up with one hand and the spine down with the other, and, as in John Bell's case, it appeared to do no good. Meanwhile nothing satisfactory could be made out by the hand; only masses of irregular and very dense coagula could be felt, from amongst which the blood came welling up. At this time the situation appeared a very critical one, and to those who were unacquainted with the operator's resources it seemed that he had only to choose between standing there for ever plugging the wound with his hand, or to withdraw it and let the patient's blood gush out.

Mr. Syme again took the knife, ran it swiftly up and down, laying freely open the whole cavity, and scooped out the clots, one of which was of enormous size; the assistants, ready with sponges, darted them into the bottom of the sac, and the serious bleeding was at an end.

All this was done with the greatest rapidity and precision, Mr. Syme stating afterwards that in taking this step he had perfect confidence in his assistants, on whose intelligent aid so much depended. The bleeding from the artery could now be commanded by direct pressure; not, however, Mr. Syme stated, when made against the bone, but when made into the sciatic foramen. The mouth of the artery was now seized with artery forceps, and a ligature carried round it and tied. All bleeding and risk were now at an end. The edges of the wound, which was directed downwards and outwards, were brought together and covered with a pledget of lint and a folded towel, but no bandage or compression was applied. The patient was under chloroform, and bore the operation well. The audience seemed surprised when Mr. Syme said that not much more blood had been lost than after a large amputation; but he reminded them that a large part of the blood which had escaped was that contained in the aneurism. In John Bell's case, the patient had fainted from loss of blood, and was supposed to be dead before the sac was laid freely open. Mr. Syme also remarked that after the firm fibrinous coagula were removed, the whole of the cavity was found to be lined by a distinct and perfectly smooth membrane.

Those who witnessed the cases in which Mr. Syme operated in the same manner for traumatic axillary and carotid aneurisms, say that the bleeding was more alarming and the difficulty greater in this gluteal case. Those who were fortunate enough to see this operation will not easily forget the scene. No surgical operation probably ever appeared more alarming; none which more required the combined and high exercise of courage, caution, self-reliance, and promptitude. Of this the large assemblage, so soon as the patient was removed, testified their feeling, contrary to the usual rule of the hospital, by greeting the operator with loud and prolonged applause. The patient continues to do well.—*Lancet, June* 22, 1861, *p.* 610.

48.—THE TREATMENT OF VARICOSE VEINS AND ULCERS BY MEANS OF BLISTERS.

Case under the care of Mr. URE, at St. Mary's Hospital.

The treatmeut of varicose veins by the application of blisters is so simple that it commends itself to notice. In the following case it was successful, whereas the previous use of needles had failed. Mr. Ure considers that the blisters cure by causing contraction of the veins and the deposition of fibrine into the surrounding areolar structures; and he has found the cure to be permanent, even in persons of advanced years. Blisters are most suitable in those cases where the vein is unequally dilated, and thicker in some parts and thinner in others than natural.

Caroline C., aged 48, a cook, married, mother of one child; admit-

ted May 23, 1861, for varicose veins of the left leg, which had been the source of more or less annoyance for seven or eight years past, accompanied with an ulcer of the size of a florin, situated on the lower third of the leg, which was painful at night. The inner saphena vein presented a dilated and tortuous condition, and here and there knots were felt along its course, feeling at times very tense, as if about to burst, associated with pain which prevented sleep at night. She was an hospital patient about a year previously, and was treated by needles, which obliterated the circulation through the distended veins for a period of three months; after which the varicose condition returned to the same extent as before. She employed water-dressing to the ulcer, and the condition of the leg occasionally improved; but whenever she over-exerted herself the ulcer became worse.

Her general health on admission was tolerably good, with the exception of occasional headache and giddiness since the "change of life." Her complexion was of a deep-red hue; pulse natural; tongue furred; bowels torpid; has suffered from piles. Ordered a blister over the ulcer, quinine mixture thrice a-day, and an electuary of senna with compound jalap powder.

May 27th. Ordered a blister, four inches square, to be placed over the cluster of varicose veins at the upper part of the leg.

30th. Condition improved; another blister to be applied to the varicose veins at the inner side of the knee, as they are bagging at that situation.

June 3rd. Improving in all respects; appetite good; dilatation of veins materially reduced. Ordered another blister.

13th. A fourth blister ordered.

24th. Leg very materially amended by the treatment: ulcer nearly well. She no longer experiences the irksome aching in the limb which she did previously.

July 1st. Sore healed. A final blister, four inches by three, applied to the lower part of the shin at the inner side, where there are still some slight remains of dilatation.

5th. The patient discharged, perfectly cured of the varicose veins and ulcer.—*Lancet, July* 20, 1861, *p.* 61.

49.—ON THE SURGICAL TREATMENT OF VASCULAR TUMOURS.

By P. C. Price, Esq., Surgeon to the Great Northern Hospital, Assistant-surgeon to King's College Hospital, &c.

[Perhaps no form of Surgical disease has called forth more ingenuity in its treatment than have nævi, and the various forms of vascular tumours. The various methods now in use are in every way superior to those formerly practised.]

For the cure and removal of superficially situated tumours depending on an anastomotic arrangement of blood-vessels, it is all important

that the means employed should be of such a kind as will insure the attainment of certain definite results.

According to the nature of the aneurismal tumour, must be the treatment employed. In some cases it will be amply sufficient merely to prevent the access of blood to the anastomosing channels. At other times, it will be necessary to excite adhesive inflammation in the substance of the vascular structure, while, not unfrequently, it will be compulsory to cause the most destructive forms of inflammatory action,—sloughing and ulceration,—ere the disease can be eradicated. And, lastly, it may be advisable to remove the tumour by the knife or ecraseur—excision—on account of the dread of the abovenamed processes being accompanied with an amount of risk, which both the patient and surgeon are anxious to avoid. The means for obtaining these various ends may be considered *seriatim*.

When it is desirable to prevent the access of blood into the anastomosing vessels of the tumour:—

Cold and astringent applications. Among the most enthusiastic advocates for the use of cold applications, was Mr. Abernethy. This distinguished surgeon believed that many forms of aneurisms from anastomosis might be considerably lessened in size, if not absolutely cured, by cold liquids, locally and continuously applied. The diminution they cause in the amount of blood entering the tumour, and the reduction in the temperature of the circulating fluid, were believed by this observant practitioner, to be sometimes sufficient to induce gradual shrinking, condensation, and obliteration. The cases adduced by Mr. Abernethy were examples of that form of aneurism which Mr. Wardrop designated *Subcutaneous Nævi*, and, in some instances, it is probable that the mere application of cold lotions, &c., may, for a time, prevent the rapid development of such tumours, even if absolute cure be not obtained. My own experience will not, however, allow me to admit that much hope is often to be entertained of a cure resulting therefrom, for I have frequently had occasion to operate on nævi of all denominations to which cold applications had been for a considerable period enthusiastically, but to no satisfactory purpose, pplied.

Dr. James Arnott recommends the treatment of certain species of nævi by the local application of ice, and it is more than probable that the use of extreme cold may, occasionally, prove advantageous, if only in arresting further growth and development.

My friend Mr. Walton informs me, however, that he has, on many occasions, attempted to obliterate capillary nævi, by the application of intense cold, but in no single instance, has he succeeded, although every endeavour was used to make the means employed successful.

Should it be desirable to employ extreme cold to any form of nævi, great care must be taken in its application, for unless such be observed, sloughing and other serious consequences are very apt to occur.

Pressure, &c. Mr. Abernethy was the first to draw attention to the advantages to be derived from the application of pressure, especially when combined with cold and astringent lotions, in the treatment of certain varieties of nævi. The forms of tumour which he considered best adapted for this kind of treatment, are the spreading, irregular subcutaneous aneurisms from anastomosis; and in support of the success which he obtained, he adduces two cases, which certainly appear to have yielded to the measures employed. One was a large spreading nævus, situated on the upper eyelid and forehead. To this, constant pressure and a solution of alum were kept applied. The other case was attended with still more satisfactory results. A large subcutaneous nævus, involving the arm, shoulder, and back, was in the space of three months almost entirely reduced by constant pressure, exercised by means of well applied bandages and strappings. There can be no doubt that this plan of treatment when judiciously applied, is often valuable in arresting the progress of, if not exactly of use in curing, some kinds of nævi. The cases in which pressure is of most avail, are those which come under the denomination of subcutaneous nævi, where the principal anastomoses of the vessels take place in the cellular tissue, and I have occasionally known these tumours yield to such means, when appropriately employed. The best way to apply pressure to isolated nævi of this description, is that suggested, I believe, by Sir B. Brodie, viz., by means of discs of ivory, or thin lead, somewhat larger than the tumour, adapted and confined with a certain force *in situ* by strips of sticking plaster. I have in one or two instances, in which all operative measures have been refused, applied pressure by means of a copper or silver coin firmly bound and kept in position by strapping or bandages.

Mr. Lacey, of Poole, has suggested that an elastic material should be used, while other surgeons have advocated the application of various ingenious contrivances, such as spring pads, plaster of Paris casts, &c. But though pressure sometimes proves of direct value in lessening, and even curing subcutaneus nævi, yet it is very often applied for a considerable space of time, without the least apparent benefit; indeed I have, on more than one occasion, known it do harm, by inducing irritation in the tumour, and inflammatory conditions of the covering, and partially implicated integument. In cases of arterial capillary nævus (Teleangiectasis) it should never be thought of, as it is very likely, even in the course of a few hours, to induce sloughing and troublesome hemorrhage. I may quote the following case, (selected from among many) as showing how the application of pressure is sometimes not only of no avail, but positively injurious. In the month of May last, I was asked by Dr. Hadaway, to see a child about eighteen months of age, the subject of a subcutaneous congenital nævus, the size of a florin, situated over the right brow. Futile attempts had been made to destroy it with nitric acid. Subsequently, pressure was applied for some weeks by means of a piece of metal, confined *in situ*,

by sticking plaster. This occasioned very considerable pain, great congestion of the neighbouring veins, and, in addition, seemed to irritate and cause rather an increase than diminution in the disease.

The growth appearing to be fed by one or more arteries of medium calibre, I strangulated it with a ligature passed through its base, in the manner to be presently described, taking care to avoid including any portion of skin in the threads. In the course of a week, the nævus sloughed away, and a small granulating surface remained, which rapidly cicatrized.

Pressure may sometimes be conveniently employed to reduce the size of a nævus, ere more definite means are practised. For whatever purpose it is used, it is always necessary to bear in mind, that a well regulated and gentle compression is far more likely to lead to diminution, and, perhaps, to obliteration, than one less discriminatively and carelessly applied.

[In the treatment of these cases it may be necessary to resort to *deligation* of the largest vessels, especially when compression leads to marked cessation of the flow of blood. For this purpose the metallic pin and the twisted suture may be most useful; but in general other means must also be resorted to. To cause inflammation in the vascular structure of the tumours is one of the most successful means of curing them.]

On looking through the records of surgery, abundant evidence will be found of the great attention that has been bestowed on the treatment of vascular tumours by the production of more or less intense inflammatory changes. With what amount of success various plans capable of producing such changes can be employed, may be advantageously noticed.

In that form of disease which is termed capillary aneurism, considerable good may oftentimes be effected by the use of certain applications which, while inducing certain mild inflammatory and other changes, do not lead to marked destruction of component tissues.

It sometimes happens that the mere application of certain astringent and slightly caustic solutions, causes a complete metamorphosis of the vascular structure. *A strong solution of alum* or *perchloride of iron* has not unfrequently been found sufficient to cause shrinking and withering of the red vascular tissue of a teleangiectasy.

I have succeeded in obliterating such forms of vascular growths by one or two applications of a moderately strong solution of *iodine*. In one case, an infant, a patient at the Great Northern Hospital, this plan answered every expectation. Two small vascular nævi on the chest, entirely disappeared after the second application of the tinct. iodinii co. No actual destruction took place, although, from the shrivelling and shrinking induced, a small white scar resulted.

I have seen a case in which my friend, Dr. Gibb, of Portman-street, used iodine to a capillary nævus of large size, situated on the back of the arm. The result is all that could be desired.

But while this plan of treatment sometimes succeeds, it will occasionally be found to fail, although every care and attention be bestowed. Under such circumstances, other applications may be tried before resorting to the use of the ligature.

Ointments of various kinds enjoy with some practitioners great repute, and are used in the treatment not only of capillary but subcutaneous vascular tumours. Hickman has strongly recommended the application of *tartar emetic* in the form of an ointment or plaster, and, and there can be no doubt that the production of certain inflammatory changes thereby induced may prove sufficient to destroy the growth.

Dr. Cumming has adduced eight instances in which "vascular nævi" were successfully treated by means of a plaster, composed of fifteen grains of tartar emetic mixed with one drachm of galbanum plaster, spread on a piece of thin leather cut accurately to the size of the tumour. About the ninth day, pustules having completely involved the diseased structure, the plaster was removed, when, in the course of time, sloughing having taken place, cicatrization followed, without the production of extensive scars.

In the subcutaneous form of aneurism from anastomosis, Mr. I. B. Brown, and other surgeons, have had recourse to tartar emetic, and, it appears, with varying results. I cannot, however, recommend this plan of treatment, as it is one which requires much care, and is very tedious and painful. Several days elapse ere the plasters can be removed, and during this period the itching and irritation are often severe. The amount of sloughing is also sometimes greater than necessary, while the scarring which results is more extensive than that which follows the use of equally or more effective measures.

A mixture of *collodion and corrosive sublimate* is often used on the continent as a destructive application to small vascular nævi. Drs. Assandri and Cösfeld strongly recommend such an application, and, in certain instances, there can be no doubt that it proves very effectual. *Collodion* by itself is sometimes used to cover small, spreading, highly vascular nævi, and its constant application occasionally leads to cure. The brilliant red colour of the growth gradually subsides, and, in the course of time, the arterial structure is replaced by a white cicatricial tissue. Dr. Browning, of Sheffield, has recorded three cases in which good results followed this plan of treatment. I cannot, however, say that I have experienced much satisfaction from its employment, for, in more than one instance, from its failure, I have been obliged to have recourse to other means.

Some small highly vascular capillary nævi may be gradually destroyed by occasionally pencilling their surfaces with *creasote*. Dr. Bujalsky has succeeded in this way in obliterating such growths.

When these various measures are found inapplicable, it may be advantageous to produce adhesive inflammatory changes in other ways.

Thus, Mr. Hodgson, Mr. Cuming, and other surgeons, many years ago, suggested the gradual destruction of congenital nævi by the *innoculation of vaccine lymph.* Such a plan has been very frequently adopted, sometimes with the best wished-for results, and at other times, with little or no advantage.

Destruction of capillary nævi by some potent caustic is oftentimes the most convenient and suitable plan to adopt. A circumscribed spreading teleangiectasy may, by being touched once or twice with *caustic potassa*, be entirely destroyed. An eschar is formed, including the whole of the vascular tissue; this, in a few days, drops off, and a granulating surface remains, which rapidly cicatrices. Great care is, however, necessary in using this caustic, as it is liable to implicate the sound tissues immediately bordering the tumour. Certain forms of limited capillary nævi may be effectively destroyed by one or two applications of *strong nitric acid.* This corrosive liquid is best applied by means of a finely pointed piece of wood. Care should be taken that not only the superficial but deeper structures of the growth are included in the application.

Although small vascular tumours can in this way be satisfactorily destroyed, still the plan of treatment is not appropriate for larger growths, especially when seated in the scrotum, &c. I have known the disease when occupying this structure, instead of being lessened by the use of acid, irritated thereby. Acid and indeed any caustic applications, cannot, with any degree of certainty, be employed for the destruction of the subcutaneous aneurism from anastomosis. If used, extensive sloughing is not unlikely to jeopardise even the life of the little patient, while, even if the disease be cured, the scar which results is often extensive.

I have seen more than one instance in which the scarring after the use of acid has been far greater than what results from the application of the ligature. I may mention the following case:—

In the summer of 1859, I assisted Mr. Fergusson to remove from the arm of the child of a lady a small nævus. About two years previously, another surgeon had, by means of acid, destroyed a similar growth, occupying the nape of the neck. The tumour was small, but the scar which resulted was nearly, if not quite, as unsightly as the original mischief.

Several applications of the acid are often necessary if much cellular or fatty tissue be mixed with the true vascular structure, and this is one reason why great discretion is needful in advising such a method of treatment.

I was consulted by a practitioner in the earlier part of the present year regarding an indurated nævus. Strong nitric acid had been used on several occasions, but each application seemed rather to irritate,

and lead to increase of the growth than promote its destruction. I accordingly strangulated it with the ligature, and, in less than a fortnight, scarcely any deformity remained.

Besides caustic potassa and nitric acid, other caustic applications may be selected, but, in appropriate cases, one or other of these will be generally found effectual.—*London Medical Review, Aug. and Sep.* 1861, *pp.* 69, 119.

50.—ON THE RADICAL CURE OF VARICOSE VEINS.

By John Wood, Esq., King's College Hospital.

A female had been subject for years to varicose veins of both legs, which were so painful that she applied for something to be done to cure them. Mr. Wood's operation consists in the application of a pair of needles, invented by him for this purpose. The needles, of the thickness of an ordinary hare-lip pin, are bent at right angles, about an inch from the head, forming, at the bend, a loop for the reception of the point of the fellow needle. The head, or blunt extremity, is also formed into a loop to serve for the purpose of retention by a strip of lint or plaster. The point of one needle is straight and spear-shaped, as in the hare-lip pin; that of its fellow is curved as in the ordinary surgical needle for sewing up a wound.

The method of application is as follows:—The skin over the vein being pinched up and raised from the vein by the finger and thumb, the straight needle is passed close under the skin, between it and the vein. The finger and thumb are then adjusted so as to raise the vein from the fascia a little on one side of the needle. The curved needle is then passed through the same punctures in the skin as the former one, but in an opposite direction and under the vein, which is thus included between the needles. The needles are then locked on to each other by passing the points of each through the loop at the bend of the other. The punctures are then protected from pressure by the interposition of a piece of lint or wash-leather, and the needles pushed in so that the loops are close to, but do not press upon the punctures. The points of the needles are then cut off with pliers. The bent head of one needle is then turned upon its fellow until it lies on the limb in the same direction. This will be found to twist the shafts across each other in such a manner as to compress laterally the included vein, and at the same time slightly to twist it. The needles are then held firm in their places by a strip of lint twisted figure-of-eight-wise, and a strip or two of sticking-plaster, care being taken to prevent all chafing. They are left in a week or ten days, until so much effusion has taken place as permanently to obstruct the current in the vein, and by progressive absorption from *acupressure on both sides* to cause complete obliteration. In the many cases treated by Mr. Wood in this manner, there has been no suppuration whatever in the track of the needles, when care has been taken to prevent

chafing or irritation. A drop or two of serum, forming a sugar-like crust, is the only discharge.

This plan, remarks Mr. Wood, possesses the advantages of being almost entirely free from pain (the skin not being included in the pressure); of giving rise to no irritation or suppuration, which is invariably the case, he says, when apparatus is used which presses into the punctures, or includes the skin, or requires subsequent twisting or tightening. It has also the advantage of a complete, equable, and constant lateral and linear acupressure without movement or disturbance; and so is insured an obliterating action on the vein, proceeding equally from the two sides. By this means, he adds, the patient is insured against the danger of putrid matters or pus getting into the circulation through the vein operated on.

In the ordinary method of passing one needle under the vein and a twisted thread or wire over the skin, it will be seen that the acupressure is on one side only, the soft tissues of the fascia, fat, and skin, being the resisting force on the other. The effect is similar to that of pressing with a stick upon a soft cushion. The coats of the vein are absorbed on one side only, and when half cut through the current may be established on the other side of the needle, and the operation fail from this cause, or purulent matter pass into the half-opened vein.

The patient after the operation remained entirely free from pain. The dressing was not disturbed, except once to ascertain the condition of the punctures on one leg, until the tenth day, when the needles were withdrawn with great ease. No suppuration whatever had occurred. The punctures were almost exactly in the same condition as on the first day. No blood followed the withdrawal of the needles. A hard line in the centre of a transverse depression was felt after their withdrawal, no swelling or hard coagulation in the veins below, the blood having found its way through the collateral venous channels.—*Med. Times and Gazette, Oct.* 12, 1861, *p.* 377.

51.—*A Bullet Lodged in the Heart for Twenty Years.*—Dr. Balch, of North Lawrence, New York, details the following rare case in the 'American Journal of Med. Science':—In 1840 a boy, aged about 14, was shot in the right shoulder, the bullet entering through the upper border of the trapezius, about two inches from the acromion. The ball, by those who examined him, was supposed to lie about four or five inches from where it entered, in a direction towards the cavity of the chest, near the sternal end of the clavicle. In about six weeks he resumed his work, and gradually quite regained his health. In 1845 he was attacked with severe pneumonia on the right side, and then was first noticed a tumultuous action of the heart, which continued to increase after his recovery—its pulsation being seen and heard at a distance of ten or twelve feet. Another attack of pneu-

monia carried him off in 1860. At the autopsy the right internal jugular was found enlarged, and the external was entirely closed, about a quarter of an inch from where it entered the internal jugular. In the right subclavian artery, at the thyroid axis, was a large ossific deposit. The heart, two or three times its natural size, was very soft and flabby; and although on the outside no scar was discernible (the pericardium was, however, very adherent on the right side), on opening the right ventricle, a leaden bullet was found imbedded in its walls.—*Med. Times and Gazette, Aug.* 31, 1861, *p.* 229.

ALIMENTARY CANAL.

52.—A RARE INSTANCE OF CONSERVATIVE SURGERY.

By Dr. J. Nichols, Savile-row, London.

The length of time that parts separated from the body retain their vitality, and consequently their capability of re-uniting with those from which they have been separated, is, I believe, not correctly appreciated either by the profession or the public; but that it is much longer than is generally supposed the following case will show.

In the summer of 1846 (I was then living in Leicester-place) a servant drove up to my door in a street cabriolet—she had come from Wellington-street, Strand,—and requested me to return with her to see her master, a gentleman of more than seventy years of age, who had fallen down and seriously injured himself. Anticipating some occasion for strapping, lint, &c., I stayed long enough to provide myself with them, and then returned with her as quickly as the crowded state of the thoroughfares would permit. On arriving at the house I was hurried up to the drawing-room, where I was met by my patient, who, holding a handkerchief to his face, said, "Doctor, I have cut my nose off" I was at that moment rather incredulous, but his daughter soon removed all doubt as to the nature and extent of the injury by showing me the separated portion, which she had picked up from the floor. It was black, cold, and covered with grit and dirt. On examining the wound, I found that the whole of the fleshy end of the nose, together with the alæ and septum, were clean cut away, and the white end of the cartilage exposed. The upper lip was divided transversely throughout its whole extent, and hung down over the mouth. It appeared that this gentleman, on going upstairs, had stumbled near the upper step, and, trying to recover himself, had fallen forwards against a wooden flap placed at the drawing-room door, the sharp edge of which had come in contact with his nose, first compressing it and then separating it from the face.

For a moment I hesitated what to do, but thinking the separated part would be as good a dressing as any other to the exposed surface, and that the patient's hope (though I had none) of its reunion would

give time for him to reconcile himself to its ultimate loss, I determined on readjusting it. This was easily enough done. The grit was washed from it, and, being carefully replaced, it was retained *in situ* by adhesive straps. The edges of the wound of the lip were brought together, and kept so by similar means.

On calling the next day, I observed that the end of the nose—which I had purposely left exposed—had lost the deep-black colour that it had when replaced, and bore evident signs of circulation going on in it. There was no discharge from the wound. On the next day, appearances were the same; and on the third, the dressings were removed, when I was as much astonished as gratified to find that union had taken place throughout the whole extent, and the scar that was left was scarcely perceptible. The lip had also united.

Now the time that had elapsed between the separation and the readjustment of the divided parts could not have been less than three quarters of an hour—an important fact, the knowledge of which cannot be, in my opinion, too widely disseminated; and when I reflect on the numerous cases of mutilation, the result often of drunken violence, that appear at our police-courts, in which no effort is made at restoration, I take some blame to myself for not having published the case before. The late Dr. Addison was called in to see some member of this family, and I showed the case to him. He examined the course of the cicatrix very carefully, and observed, "You ought to publish the case."—*Lancet, Aug.* 24, 1861, *p.* 195.

53.—*Ulcerated Throat.* By Ellis Westcott, Esq., Westminster Hospital.—I have found the undermentioned of great value, and never knew it to fail. The following lotion to be applied night and morning with a camel's hair brush:—Nitrate of silver, ten grains; pure nitric acid, three drachms; to five drachms of distilled water. Also a gargle, to be used frequently, consisting of gallic acid, ten grains; tincture of capsicum, half a drachm; to six ounces of compound infusion of roses. And a tablespoonful of the following mixture to be taken three times a day:—Dilute nitric acid, two drachms; syrup of oranges, half an ounce; to eight ounces of infusion of chiretta.

If these be persevered with for a short time, I am convinced that a perfect cure will be established.—*Lancet, May* 18, 1861, *p.* 500.

54.—ON THE MODE OF OPERATING FOR THE RADICAL CURE OF HERNIA.

By John Wood, Esq., Surgeon to King's College Hospital.

[The patient in this case was a sailor; the hernia was a right oblique scrotal hernia of ten months' duration.]

An incision directed obliquely downwards and outwards was first made through the skin of the scrotum over the middle of the hernial

sac, with a small narrow-bladed knife. The edges of the skin were then separated from the subjacent fascia by carrying the blade of the knife flatwise under the skin to the extent of three-quarters of an inch from the margin of the incision. The denuded fascia was then invaginated by the fore-finger of the right hand into the inguinal canal, carrying the sac before it. The lower border of the internal oblique was then felt for, and raised upon the finger. The curved hernia needle was then carried along the finger at its thumb side (*i.e.*, towards the patient's linea alba) and passed through the conjoined tendon from behind forwards till the point of the needle was seen to raise the skin of the abdomen about one and a-half inches above the pubis. The skin of the groin was then drawn inwards as far as possible, before the needle was pushed through it. A piece of stout copper wire silvered, and tapered off at both ends to a flexible degree of thinness was then attached by one end to the eye of the needle, and drawn back with it through the scrotal incision, and then detached and left in the wound. The invaginating finger was then carried outwards behind Poupart's ligament, or external pillar of the ring, as high up as possible. The aponeurotic structures were then lifted upon the finger, and the needle passed through them on the finger along its opposite border (*i.e.*, that nearest the ilium of the patient). The point of the needle was carried through the same aperture in the skin of the groin as in the first instance. The other end of the wire was next fixed to the needle, drawn back with it into the scrotum, and also detached. The sac of the hernia, at the scrotal incision, was then pinched up from the cord behind by the thumb and forefinger of the left hand, and the needle passed across it in front of the cord, in the same manner as in passing a needle for the cure of varicocele, but within the limits of the scrotal incision. The inner extremity of the wire (*i.e.*, that which included the conjoined tendon) was then fixed to the eye of the needle, and drawn through the sac across the front of the cord. The two ends of the wire was next drawn down till the loop or bend was close to the upper puncture, the loop being held by the forefinger of the assistant. The ends of the wire were then twisted firmly down upon the fascia and sac in the lower puncture, and cut off at a point about five inches distant from the skin. By traction upon the upper loop the twisted ends of the wire were then drawn up into the inguinal canal, invaginating the sac and fascia, and leaving, when drawn tight, about two inches protruding from the lower puncture. The upper loop of wire was next twisted firmly down into the puncture in the skin of the groin until the sides of the canal were drawn firmly together. The lower or cut ends of the wire were then bent into a small hook, and curved upwards, so as to meet the upper or loop end curved downwards to meet it in a corresponding degree. The hook was then fixed into the upper loop, a pad of lint inserted between the wire and the skin, and a spica bandage and compress put over all, so as to exercise firm pressure upon the posterior

wall of the canal. The spermatic cord is left behind the wire with a small portion of the neck of the sac covering it. This part of the sac is obliterated by the pressure of the wire backward upon it.

The above method of operating is the one now practised in most cases of scrotal hernia by Mr. Wood. It is simple and easy in application, and has proved very decidedly successful in the cases in which it has been employed hitherto. There is less pain and much less discharge than in the cases where the ligature thread and hard compress were employed. The use of wire also gives the valuable advantage of enabling us to employ direct contact and pressure upon that part of the posterior wall of the hernial canal and sac close to the cord which is not included between the two ends of the wire, and so to effect its complete closure by adhesion at that point.

Mr. Wood has now operated in thirty cases, and up to the present time has had only six failures. The first case was operated on more than three years ago, and was shown at King's College Hospital a short time ago quite cured, and wearing no truss.

Since the operation the case has done uncommonly well. There has been remarkably little suppuration. Some swelling of the testis showed the effect of the pressure in the inguinal canal. The wires were untwisted and removed with great ease and little pain sixteen days after the operation. The patient is now almost ready to be discharged (three weeks after the operation) with great consolidation of the parts.—*Med. Times and Gazette, June* 1, 1861, *p.* 578.

55.—ON A NEW OPERATION FOR THE RADICAL CURE OF HERNIA.

By Prof. Chisholm, of South Carolina.

This operation, by Prof. Chisholm, of South Carolina, is thus described by one of his pupils:—"These and other considerations have induced Dr. Chisholm not to rest satisfied with the successes gained even by his modification of Wood's operation, but to add a still greater improvement, simplifying the entire procedure, and obviating nearly all the objections which have been, or may be urged to Wood's.

"The new operation is as follows: The scrotum having been invaginated upon the finger—as the only mode of guiding the needle in its passage—a long, strong, curved needle, fixed firmly in a handle, and armed with silver wire, guided by the finger, transfixes the scrotum at the apex of the invaginated portion, passes through the internal column, and appears through the skin of the abdomen, when one end of the wire is drawn out. The point of the needle is then drawn backwards, and disappears again in the canal. Its direction is then changed. Whilst still embedded in the scrotum, and guided upon the finger, its point is made to traverse the external column of the

ring, near Poupart's ligament, lifting the skin of the abdomen. By gliding the skin upon the needle, the point appears through the small puncture made by the first passage of the needle, when the other end of the wire is seized, the needle is unarmed and withdrawn through the scrotum. The finger is now removed from the canal, and the two ends of the wire being drawn upon the loop, it dissects the cellular tissue up to the columns, which it hugs closely. By twisting the ends of the wire the columns are felt approaching, until they are brought into such close apposition as to allow nothing to pass between them—the spermatic cord in its exit filling up all the available space remaining of the ring. When the ring is felt closed, the twisted wire is drawn firmly outwards, and clipped off as close as possible to the skin, so that, when traction on the skin of the abdomen is removed, the gliding back of the integuments to their normal positions conceals completely the ends of the small loop of silver wire. The scrotum has already fallen back to its pendent position, and the only trace of an operation having been performed is in the two small punctures, one in the scrotum and the other in the abdomen, which require a careful search to find them, and which will heal up in a few hours, hermetically incarcerating the silver wire.

"A moderate inflammation follows the operation, without much pain or swelling, and without any fear of suppuration. The wire is soon embedded in a lymphy deposit, which will not only enclose it, thus isolating it from the tissues, but, at the same time, agglutinates the columns together as an additional security to the success of the operation. The patient is kept quiet in bed four or five days, opium being given to insure rest and prevent action of the bowels. When the inflammatory stage has passed, a cathartic is administered, and the patient can quit his bed, resuming his occupation in a few days. The silver wire remains a permanent application. An essential element in the success of the operation is that the loop encircle the columns of the ring near their points of attachment to the pelvis. Otherwise, the columns cannot be approached, the ring remains open, and the results can only be negative. If this step of the operation be carefully followed, a radical cure can be nearly guaranteed."

The advantages of the operation are the short time the patient is detained in bed, the slight amount of inflammation produced, the dispensing with the use of a truss, and the security against relapse, consequent on muscular effort—the reliance being placed, not on newly-formed adhesions, but on the silver wire. When the protrusion is large and the ring voluminous, several points of suture may be applied through the same punctures. The objection that the wire will always act as a foreign body is shown by experience to be unfounded, as it may remain harmlessly embedded in the tissues for any length of time. Four cases only have as yet been so operated upon, and that only since Nov. 1860.—*American Medical Times*, No. 5.

To the above we may append an account of a mode of operating

found useful by Dr. Agnew, of the Philadelphia Hospital, and described in the North American Medico-Chirurgical Review (January, p. 117), in the following terms:—"The instruments necessary for the operation consist, first, of two semi-cylinders of steel, 3 inches long and $1\frac{1}{4}$ inches in circumference, which can be separated from each other by a screw in the handle of the instrument, and on the internal face of the lower blade of which are two parallel, longitudinal grooves: secondly, of a spear-pointed needle, slightly curved at the extremity, and supported on a bone handle; and, thirdly, of several stout needles of from 2 to $2\frac{1}{2}$ inches in length.

"A portion of the scrotum is carried in followed by the metal cylinder, and thrust up to the internal ring. The blades are now separated by means of the screw, and the long needle, armed with a thread of silver wire, is carried along one of the grooves to the upper end of the cylinder, and then made to pierce the exterior parietes of the inguinal canal. The thread is then removed from the needle, the latter withdrawn, and the other end of the wire passed through its eye, when it is passed along the other groove and made to emerge a short distance from the first. It is again unthreaded, and after its removal the two ends of the wire are drawn up tightly and twisted over a small roll of lint. This effectually holds up the small plug of integument to the very summit of the canal, and as the silver thread manifests but little disposition to ulcerate out, it may be allowed to remain for a considerable time. The second and most important step of the operation consists in screwing the handles of the instrument completely together, (thus separating the blades in the canal to their greatest possible extent), and then, carrying across the canal, between the blades four or five threads, at equal distances from each other. The first thread should be composed of silk, and be introduced as near the internal ring as possible. The remainder should be of silver, the last one being close to the external ring. These transverse threads can be lodged in the canal with great accuracy by this method, the cord being protected from injury by the posterior blade of the instrument. The latter being withdrawn, the patient is confined to his bed, the parts being protected by a compress and roller, and the wires should be removed as soon as the plastic exudation has bound the invaginated plug to the walls of the canal, which in the cases operated upon required twelve days."—*Medical Times and Gazette, March* 30, 1861, *p.* 339.

56.—COMPLICATED CASE OF OBLIQUE CONGENITAL INGUINAL HERNIA.

By Dr. William J. Wilson, Surgeon to the Clay-Cross Collieries, Derbyshire.

On the 16th of April, 1861, at two a.m., I was summoned by Dr. Houghton, the assistant-surgeon to the Clay-cross Works, to a patient

suffering from strangulated oblique inguinal hernia, whom I found drunk, collapsed, pulseless, and vomiting, apparently in great pain, and very unruly. On examination, I discovered a firm, oval tumour, the size of a hen's egg, in the left groin, and considerable fulness along the course of the inguinal canal. I entertained doubts as to the character of the tumour, but the patient not being in a position to give me its history, and the bystanders knowing nothing more than "that it had been down once before, and had been put back without cutting," I did not feel justified in leaving my drunken patient with such a suspicious tumour in his groin.

Dr. Houghton at once administered chloroform, and it acted as a direct stimulant to the nervous system and to the heart's action, as, indeed, I believe it almost invariably does in collapse the result of injury. The patient having been brought fully under the influence of the chloroform, I applied the taxis, but without success, and then proceeded with the usual operation for the relief of oblique inguinal hernia. I found it necessary to divide some constrictions at the internal abdominal ring, after which a portion of the tumour was returned into the abdomen with an unmistakable gurgle. A tumour, however, still remained in the groin, between the scrotum and the external abdominal ring. I re-introduced my finger into the inguinal canal, and found only the spermatic cord, and that the communication with the abdomen was quite patent. Now came the question—What is this tumour? Not an undescended testis, for there are two in the scrotum. Is it, then, a hydrocele of the cord? I applied the knife, divided a fibrous membrane, and then there exuded a number of fine, vermiform, yellowish filaments, each accompanied by a congested vein; and on squeezing the testicle (?) in the scrotum, these filaments and veins protruded all the more from the cut in the inguinal tumour. I now felt certain that I had cut into a portion of the testis, and believed that the inguinal tumour was the globus major of the epididymis, enlarged by disease, and that the vascular filaments were coni vasculosi. The gut being fairly returned, however, I felt only a temporary pang at having wounded the testis, and was content to labour for a few days under the imputation of having operated on a testicle in mistake for a hernia.

Eight hours subsequent to the operation I obtained the following history from the patient: He has always had *two* testicles in the scrotum, and has remarked that the left one (corresponding to the groin operated on) has always been a little smaller than the other. Never observed a tumour in his groin, except on one occasion, when "it came down" while he was lifting a weight. It was returned by my late assistant, Mr. Carter; and he had leeches applied to his abdomen, and took pills and medicine. On the present occasion, he remembers sneezing, then "feeling something give way in his groin," which caused such great pain that he fell to the ground. "Drunk and incapable," there he lay until found by his friends, at two a.m.

The history was simply that of a hernia of the gut. Had I then erred as to the tumour left in the groin? Had I left a lump of omentum there? I felt certain that I had not erred, and that my patient's history was not correct. Both Dr. Houghton and myself had passed our fingers into the abdomen; both had felt that the inguinal canal contained nothing but the spermatic cord; both had seen the spermatic ducts (?) ooze through the cut in the inguinal tumour when the testis (?) in the scrotum was squeezed; both of us had felt the tumour, and traced the spermatic cord passing into it. How could we be mistaken? The sequel will show how far we were correct in our diagnosis.

Nine P.M. Swelling and intense tenderness of the testicle (?) in the half of the scrotum corresponding to the groin operated upon; pain and tenderness along the inguinal canal and over the lower part of the abdomen; anxious expression; pulse 120, small, and wiry; great nausea and faintness; slight bilious vomiting; no action of the bowels. I diagnosed *orchitis*, consequent on the puncture of the portion of testis (?) in the groin; and peritonitis, the result of the strangulation of the bowel and of the operation; and I saw in this orchitis (?) a confirmation of my opinion as to the nature of the inguinal tumour. Ordered twenty leeches to the tender parts; calomel and opium every two hours; and the effervescing citrate of potash with prussic acid.

17th. The peritonitis has become more general, but the scrotal tenderness is much less marked. Ordered a repetition of twenty leeches, and to continue with the calomel and opium. Flatus has passed freely per rectum, but no stool. Vomiting continues, but the ejecta have not the slightest suspicious appearance or smell.

18th. Dr. Booth, of Chesterfield, kindly met me in consultation, and to him I stated every particular relative to the history, the operation, and the subsequent treatment of the case; the main points of my statement being, that a knuckle of intestine had been returned, completely and satisfactorily, but *no sac opened*, and I believed there was none; that the remaining inguinal tumour was *part of*, if not *the* testis; and that I had satisfied myself of this by opening it. I asked whether the symptoms called for any further operative interference, Dr. Booth replied that *the testicle was certainly in the scrotum;* but that he would not hold himself responsible for what was in the groin. Believing my statement as to the complete return of any abdominal protrusion that might have been present, he did not consider the existing symptoms more urgent than frequently occurred after the operation for hernia, and he should not advise an operation, but should rely on calomel and opium, fomentations, &c.

19th. Ten A.M.: Has passed a tolerable night, and is improved in every respect. The abdominal and scrotal tenderness has almost entirely disappeared; the wound in the groin is sloughy and painful. I administered a copious enema of warm water. It returned, in about half an hour, slightly discoloured and smelling very offensively.—Ten

P.M.: Vomiting has been very urgent for the last two hours, and the ejecta have a fecal appearance.

20th. Vomiting decidedly fecal and very copious. Bowels not moved. Complains of great pain about the umbilicus and left groin. The patient insists that "there is an obstruction," and prays that it may be removed.

I now began to feel that a second operation was imperatively demanded, and that in all probability we should find a knuckle of intestine had again been forced into the inguinal canal (rendered unusually patent by the recent operation) during the act of vomiting, no pad having been applied in consequence of the tumour left in the groin.

21st. Two P.M.: In the presence of Dr. Booth, Dr. Houghton, Mr. G. O. Siddall, and two medical students, I turned up the flaps made at the previous operation, and exposed a tumour, of the size of an egg, thickly coated with recent lymph. After scraping off some of this lymph we found the tumour to consist of two distinct parts, the upper portion being a knuckle of inflamed intestine, and the lower the testis (?). The intestine was flaccid, and overlapped the upper portion of the other tumour. I enlarged the incisions made on the previous occasion, so as to expose the whole of the inguinal canal, and found that by simply breaking down the recent lymph which connected the bowel with the canal and the internal ring I could readily pass my finger into the abdomen. Observing this, together with the flaccidity of the bowel, I felt that the hernia was *retained* in the canal by the inflammatory products rather than *strangulated at its neck.* Partly with my finger, partly with an edgeless knife, I broke through the remaining adhesions, and then returned the bowel with the greatest facility. The gentlemen present now examined, with their fingers, the tumour left in the groin and the one in the scrotum, but without satisfying themselves as to which was really the testis. The prevailing opinion was that the *testis was in the scrotum.* Each tumour felt as though a "cord" passed into it. The examination was merely tactile, the inguinal canal having been ascertained to contain nothing but the spermatic cord, and the communication with the abdomen to be quite free. The vomiting and sense of "obstruction" were relieved by the operation; but the patient died thirty-six hours afterwards, in a state of collapse, no hiccough having occurred, and the bowels not having acted.

Autopsy.—I regret that a thorough post-mortem investigation was not permitted, and that I was compelled to pledge my word that none other than the parts previously operated on should be examined, and that these should not be removed from the body. Dr. Booth assisted at the examination. To the eye and to the touch there appeared to be three testicles, two in the scrotum and one retained in the left groin. The inguinal canal and groin were now laid open. The proper inguinal canal contained, apparently, merely the cord, and was lined with a layer of recent lymph. In the groin was a tumour coated with

lymph, which lymph was found to conceal a thin layer of blood-vessels and (what looked like) nerves, connected by a very fine membrane. This layer passed into the scrotum. The tumour proved to be an undescended testis, with its epididymis and cord. So far, then, with the exception of this nervo-vascular (?) membrane, the relation of parts was such as would ordinarily exist in a case of congenital inguinal hernia prior to the descent of the testis into the scrotum, and prior to the occlusion of the processus vaginalis from the general peritoneal cavity. The testis and epididymis were quite healthy, and neither of them wounded. No doubt "the fibrous membrane" covering the testis, which I divided on the occasion of the first operation, was the peritoneum, or, to speak strictly, the processus vaginalis, thickened as a hernial sac becomes thickened when long exposed; and the filaments which I evacuated, and mistook for "coni vasculosi," were a portion of the nervo-vascular(?) membrane which I have alluded to. I explain the reason of my not exposing the testis thoroughly at either of the operations thus:—The incision through the skin was a long one (and subsequently enlarged by a shorter one made at an acute angle.) The deeper incisions were confined to the *upper portion of the wound*, and the intestine was reached without opening any sac of which I was cognizant. This, of course, is usual in congenital hernia. Having returned the gut thoroughly, the testis felt (through the remaining undivided layers of tissue) like a distinct tumour, as indeed it was, and I did not feel justified in doing more than making such a puncture or (if necessary) incision as should satisfy me as to its nature. I felt satisfied that the filaments which exuded on making this puncture were "coni vasculosi," and so made no further dissection. I was wrong, and no doubt my explanation may not be considered satisfactory. I rest satisfied with having done my duty to the best of my ability. It is impossible to place "old heads upon young shoulders" with any prospect of forming a successful combination. The testis was now removed from its position. Behind it we found a corrugated, crumpled-up, thick substance, coated with fibrine. The canal became contracted immediately below the testis, so as to form a neck, through which the substance just named passed, as also the film from the front surface of the testis. The finger could be passed through this contracted portion of the inguinal canal into the scrotum, and here the canal expanded into a globular cavity, which was filled with some membranous substance, which we presently examined. We laid open the scrotum, and exposed a glistening, oval, fibrous body, about the size, and having really somewhat the appearance, of a testicle. It had a neck too, which during life, and even on tactile examination after death, passed very well for a spermatic cord. We opened this curious body, and found it to be a strong fibrous envelope, about the eighth of an inch in thickness, lined with recent lymph, and containing a globular something, also coated with lymph. This indefinite something we found to be an undeveloped, highly vascular portion of

omentum, crumpled up like a veil into a roundish mass, with a process or neck passing from it through the contracted portion of the capsule; the main portion then passing behind the testis, and becoming firmly attached to the spermatic cord and to the posterior wall of the inguinal canal, whilst a thin film passed in front of the testis, and contributed to form the "coni vasculosi," which I evacuated at my first operation. The fibrous envelope became much thinner at its neck, and gradually lost itself in the lining of the inguinal canal.

Remarks.—I take it that in this case the communication between the general peritoneal cavity and the processus vaginalis lining the inguinal canal and scrotum had never been closed; that a portion of omentum passed during fœtal life into this vaginal process prior to the descent of the testis; that the fibrous envelope consisted of peritoneum, fascia transversalis, and intercolumnal fascia, blended together and developed into a strong covering for the protection of the exposed portion of the peritoneal cavity and of the misplaced omentum; and that the testis remained in the groin, because its arrival in the scrotum had been anticipated and its place occupied by the protruded omentum. The canal of the processus vaginalis, when examined after death, was found to be contracted, first, at the internal abdominal ring; secondly, just above the testis; and, lastly, at the point where it formed the neck of the scrotal tumour. The contraction at the internal ring no doubt explains the reason why the hernia of the gut was not constant. I would again allude to the omentum. Is it possible that the testis passed, during fœtal life, between the layers of the great omentum? It certainly was between them when we examined the body. The appearance, too, of this omentum was very deceptive and peculiar. The thin film in front of the testis (in the groin) looked just like a number of veins and nerves connected by fine fibro-cellular tissue, but coated on the external surface with soft, recent lymph. Posteriorly, this fine, vascular membrane was, as I have said, corrugated and crumpled up into a rounded substance about the size of the little finger, which expanded above and became firmly connected with the spermatic cord and the wall of the inguinal canal, and likewise expanded below so as to form the globular mass found within the fibrous capsule in the scrotum.

I must again express my regret that we were not allowed to open the abdomen; and I shall be most happy to afford information on any points connected with the case which my readers may think it worth while to inquire about.—*Lancet, June* 15, 1861, *p.* 583.

57.—PERFORATION OF THE INTESTINE.

By M.M. Velpeau and Verneuil.

When reporting the discussion which recently arose in the Society of Surgery, on the course to be adopted after the operation for hernia, when the intestine is perforated, we ventured to assert that whatever

the extent of the perforation, the reduction of the intestine with or without suture must be deemed irrational; nor are we disposed to think that the exceptional instances adduced in France and England in support of a contrary opinion can invalidate the teachings of reason, backed by observation.

At the meeting of March 13th, M. Velpeau related five cases in which he had returned the perforated bowel into the abdomen with success. In the patients cured by this method, the eminent Professor stated that the intestinal convolution was healthy, and presented but a minute aperture. In one instance the perforation had probably been consequent on tractions exercised on the incarcerated intestine, and a second case was followed by stercoral fistula, which closed spontaneously. Notwithstanding these felicitous results, M. Velpeau did not present, as an example to be followed, the course he had adopted under a fortunate concourse of circumstances which may not again be met with. "Indeed," said he, "one consideration must not be lost sight of: the operation for strangulated hernia may be descanted on at much length, all the known cases may be brought forward, and all probable incidents as far as possible delineated; and yet it will remain impracticable to lay down absolute rules for the guidance of the surgeon. Each operation presents its own special difficulties, which the practitioner is called upon at once to decide on. No invariable maxims can be propounded applicable to all cases; each stands by itself, and requires to be considered separately."

M. Velpeau, it may be seen, is but moderately partial to artificial anus. He conceives it is much too lightly spoken of. In many instances when well established it closes spontaneously; but in many also it becomes an incurable infirmity, not to mention the disastrous occurrences which are occasionally observed before consolidation is effected. We should not slur over the disadvantages, or even the perils, attendant upon the formation of artificial anus, and we cannot look upon this method as the final expression of surgical progress. But we must not forget a view which is, for the present, paramount; namely, that of the various primary or secondary contingencies which endanger the life of patients after the operation for strangulated hernia, when the bowel is perforated, the most immediately formidable is stercoral effusion. Hence the precept of non-reduction is, unquestionably, that which is most applicable to the greater number of cases, and must continue to prevail until a careful inquiry into the instances of exceptional success resulting from the opposite procedure shall have thrown light upon the circumstances which may, in such cases, lead to a cure without resorting to the formation of artificial anus.

The necessity for this aperture being now granted, its establishment must be proceeded with so as to meet the objections which have been raised to the method; and none better than M. Verneuil has pointed out the indications to be satisfied under the circumstances.

"The intestine must be secured so as to obviate its sudden return into the abdominal cavity, and to permit it, on the contrary, to retrocede by the slow and gradual mechanism instituted by nature in fortunate cases. Both extremities should be fixed in appropriate relative positions, were it but to promote their future union, or to enable the surgeon to discover them again, without difficulty, for the purpose of anaplastic operations, should they not have a tendency to unite by the unaided agency of nature.

"The whole of the suspicious portion of the bowel should be left in the external wound, and there secured, whether the perforation occupy the summit, the body, or the pedicle of the convolution: hence the necessity of a most careful exploration of the intestine, and of exposing the part which has suffered constriction.

"Not only should no perforated or mortified spot be returned into the abdomen, but if any portion of the bowel appears to be in a state of vitality, it should not be reduced, as it might subsequently give way. As the distance to which gangrene of the intestine may extend around a perforation or a mortified spot is uncertain, it is always safer to keep it in the external wound, and there fix it with every, even apparently unnecessary precaution.

"The extent of bowel maintained outwardly should be such as to leave between the peritoneum, at the back of the ring, and the artificial aperture, a distance sufficient to obviate all infiltration, and to prevent the inflammatory action, which must occur in the wound, from spreading to the serous lining of the abdomen.

"The intestinal aperture should be adequate to the free escape of the contents of the intestine, the obstruction of the bowel being the chief danger of strangulation, and the essential cause of the local or general symptoms, and the efficacy of the artificial anus being entirely proportionate to the freedom of the evacuations.

"Therefore, without taking into account the contradictory and exclusive precepts of authors, the surgeon will use the knife, guided merely by the view of securing the free egress of the discharges. Incision of the ring will, in most instances, be useful for this purpose, and also for the complete inspection of the strangulated convolution and the proper securing of the intestine in the wound.

"The greatest precautions are indispensable during the operation to prevent passage into the abdomen of stercoral matter, injurious gases, and gangrenous fluids. The external wound will be kept open, and proper attention must be paid to the unimpeded issue of the contents of the intestine during the necessary time."

These precepts show that many of the procedures hitherto recommended for the treatment of strangulated hernia with gangrene of the intestine are imperfect and unsafe. That which M. Verneuil conceives to be preferable, consists in the suture of both ends of the intestine, or of the margin of the perforation to the wound of the skin, the diseased portions being removed or not as the case may require. As

this plan would, however, increase the chances of permanency of the artificial anus, it would be advantageous to prevent, if not the formation of the spur, the size of which depends upon the extent of the destruction of the intestine, at least the union of the mucous membrane of the intestine with the integument. For this purpose M. Verneuil advises a margin, destitute of epithelium or epidermis, to be left between these parts which may granulate, become gradually contracted, and finally bring about the occlusion of the abnormal orifice. Careful dressings and, if necessary, superficial excisions would doubtless be further conducive to this desirable result.—*Journal of Practical Medicine and Surgery.—Med. Circular, Aug.* 21, 1861, *p.* 135.

58.—ON SOME DISEASES OF THE RECTUM.

By James Lane, Esq.

Mr. Lane drew the attention of the Harveian Society to the subject of hemorrhoidal disease, his remarks being founded on the experience obtained during a three years' connexion with St. Mark's Hospital for Diseases of the Rectum. Subdividing these complaints in the usual manner into external and internal, he commenced by briefly describing the nature of the two conditions, giving his views with respect to the structures involved, and an account of the leading symptoms in each. In speaking of the treatment of swellings situated external to the anus, he said that when they were caused by the rupture of a dilated hemorrhoidal vein, the best and most effectual remedy was an incision, to allow the coagulum to escape; and that when they were formed by enlarged and inflamed portions of skin, excision with the knife or scissors was the only method which could be depended upon for effecting a permanent cure. He strongly condemned the use of the ligature, which, notwithstanding all that had been written on the subject, he still found to be not unfrequently employed in such cases. With reference to the *curative* treatment of internal hemorrhoids, two principal methods had been practised—excision and the ligature; to which might be added a third—namely, cauterization with nitric acid. Excision, which was formerly the common practice, possessed many and great advantages: the tumours could be readily, rapidly, and completely removed; the operation was attended with less pain, both at the time and subsequently, than was occasioned by the ligature; it produced less irritation, and was followed by more rapid recovery. It had, however, one very serious disadvantage—the liability to dangerous hemorrhage, enhanced by the great difficulty in getting at the bleeding surface to arrest it. This danger had been felt to be so great as to outweigh all its other advantages, and had led to its almost complete abandonment in the present day, and to the substitution of the ligature in its place. In every respect, however, except in avoiding the risk of bleeding, the opera-

tion by ligature was inferior to that by excision. Such being the case, it would be admitted to be a great desideratum if an operation could be contrived which should combine the principal merits and avoid the principal demerits of both. Such an operation had been devised by Mr. Salmon, and had been practised by him with great success for many years. He did not think this operation was by any means so generally known as it deserved to be, and one of his reasons for bringing the subject of hemorrhoids to the notice of the society was for the purpose of explaining its nature. It might be said to be a sort of compromise between excision and the ligature. It consisted of a separation of the hemorrhoidal tumour from the subjacent parts for about the lower three-fourths of its extent, leaving it attached by the remaining upper fourth, which was then included in a ligature. The tumours were drawn down by means of a hook with four prongs, contrived for the purpose, and the division of the lower part of them was made with scissors. These structures were always supplied by vessels which descended from above, close beneath the mucous membrane, and the trunks being necessarily included when the upper part of the tumour was tied, all danger of bleeding from the divided branches was avoided. By this proceeding, the smallest possible amount of tissue was included in the ligature, and what was included was as far as possible removed from the anus. This latter circumstance he believed to be a great advantage, for the membrane an inch within the anus possesses little of that acute sensibility which characterizes the integument close to that aperture.

The operation being thus far completed, another point of importance was the management of the œdematous and everted collar of integument which always surrounds internal hemorrhoids when they are protruded. The protusion from within is necessarily acccompanied by an eversion of the skin with which it is continuous, and it by no means follows that there is any superfluity of this latter tissue. To remove all the apparently superfluous skin would be followed by a greater evil than the disease itself—an irremediate contraction of the anal aperture after cicatrization had taken place. The rule which he had found it desirable to follow was, to remove all irregularities and all pendulous portions of skin freely, but not to meddle with the circular fold which surrounds the protruded parts. It might be expected that inconvenience would in like manner result, in the interior of the rectum, from the free removal of mucous membrane, which was a necessary part of these operations; such, however, he had not found to be the case. He had, indeed, in some few cases, where the disease was extensive, seen a decided narrowing of the lower end of the rectum for some weeks after the operation; but it had in every instance gradually subsided, and all inconvenience had disappeared. The cicatrices of mucous membranes did not appear to have that tendency to contract, or to remain firm and rigid, which was found to be the case with cicatrices of the skin. In Mr. Salmon's operation, the adoption

of which he strongly recommended, he had found the subsequent pain and irritation were very much lessened; and in evidence of this he mentioned that he had very rarely found it extend so as to involve the urinary organs; whereas retention of urine, according to his experience, was the rule, rather than the exception, after the ordinary operation with the ligature.

A circumstance which he thought well worthy of note was the very slight risk attending operations on the rectum, if anything like proper care were taken in the selection and management of cases. He had during the last three years and a half performed 166 operations for hemorrhoids, 202 for fistula, and 97 for fissures, polypi, and procidentia—465 in all. He had assisted at about an equal number in the practice of his colleague, Mr. Gowland, making a total of upwards of 900 operations, the results of which had been carefully recorded. Amongst them there had not been one instance of pyæmia, and not one instance of erysipelas; in fact, excepting in four cases, there had not been a single complication at all of a serious character or deserving of notice. These four exceptions were cases of tetanus, which occurred in the spring of 1858, within a very short period of each other. Two had occurred in his own practice, and two in that of Mr. Gowland. They all followed the operation for hemorrhoids, and all four were fatal. He thought, however, that he was justified in considering them as accidental, especially as numerous deaths were occurring from tetanus in other hospitals in London at the same period, and the disease might therefore fairly be said to have been epidemic at the time. In one hospital which he mentioned, nine deaths had taken place from tetanus within a period of two months. It had never been noticed by other surgeons that operations on the rectum were especially liable to tetanus, and in his own subsequent experience, which extended to several hundred operations, he had seen no other example of it. He thought there were few operations in surgery which afforded with so much certainty such complete and permanent relief from serious disease at so small a cost of suffering and risk.

With respect to the treatment of hemorrhoidal tumours with nitric acid, his experience did not lead him to recommend it for general adoption; but neither was he disposed to condemn it indiscriminately, as some of its opponents had done. No doubt much injury might be caused, and had been caused, by its careless employment; but he had himself used it in twelve cases, and his objection to it was rather on the score of its inefficiency than on any active injury which he had found to result from it. He believed it to be well adapted for those cases in which there was an abnormally vascular condition of the mucous membrane of the rectum, without the development of large or distinct tumours which could be protruded; but, under the opposite circumstances, repeated applications of the acid were necessary to reduce the diseased parts to the level of the surrounding surface, and the treatment then occupied a much longer time, was much less likely to be

permanently successful, and was in reality much more severe than the operation by the ligature, or that modification of it which he had recommended.

Mr. Brown had tried the plan of tying internal hemorrhoids as recommended by Mr. Lane, and although it possessed many advantages, still he had occasionally found considerable contraction of the anus after their removal. He (Mr. Brown) preferred simply dividing the skin where it joined on to the mucous membrane before applying the ligatures; carrying out, in fact, the golden rule laid down by the late Mr. Copeland, "cut skin, tie mucous membrane." Mr. Brown always used ligatures of twine in preference to silk. With respect to the observation of the author that there was more bleeding from hemorrhoids in women than in men, Mr. Brown explained that it arose frequently from a suppressed action of the menstrual discharges, or from the cessation of that discharge at certain changes of life. Mr. Brown could confirm the statement of the author as to the safety of all operations of this kind, as he had performed a large number, amounting to many hundreds, and had never met with a single death or a case of pyæmia.

Mr. Lane did not altogether concur in Mr. Copeland's maxim,—always to cut skin and tie mucous membrane. He would always cut both, were it not for the danger of bleeding; and he thought the next best thing was to cut as much as was safe, and tie the remainder. He believed it to be sound surgery never to remove tissue with the ligature when it could be safely done by a cutting operation.—*Lancet, August* 3, 1861, *p.* 112.

59.—ON THE TREATMENT OF CERTAIN FORMS OF PROLAPSUS ANI WHERE THE LIGATURE IS NOT ADMISSIBLE.

By Henry Smith, Esq., Assistant-Surgeon to King's College Hospital.

In the 'Med. Times and Gazette' for Nov. 17, 1860, are some observations on the most efficient means of treating severe cases of prolapsus of the rectum where the ordinary method—by the ligature—could not be adopted. I entered at some length into the pathology of the disease, and described the varied conditions under which it is so frequently met with. It was particularly insisted upon that prolapse of the rectum was associated, in many instances, with some serious organic mischief, and that the use of the ligature in such cases would be attended with considerable danger. In lieu of the ligature I recommended the combined method of treatment—viz., the application of nitric acid to the prolapsed membrane, and subsequent removal of transverse folds of integument from around the margin of the anus. I purpose now to detail a few cases where this plan was carried out:—

A gentleman, aged 54, was sent to me on Dec. 7, by Mr. Fereday, of Dudley, who, in a letter which the patient brought, informed me of the existence of some organic disease, and the appearance of the gentleman at once indicated something more serious than a prolapsus of the rectum, however serious. There was a lividity of the face, and an amount of dyspnœa, which betokened something wrong with the heart or lungs. He informed me that, for several years, he had suffered from prolapsus of the rectum, which had been preceded by hemorrhage for some time. This symptom, however, had passed by. During the last two years his prolapsus had become so much worse, that he was in almost continual suffering, the bowel not only protruding at the closet, but coming down when he began to walk about. The pain after his evacuations was very severe, and he was not always able to control them. Moreover, as soon as he returned the protruded bowel it came down again, and the consequence was, that he could not sit without pain, and has latterly been rendered quite wretched from this cause alone, without taking into account his other ailments. On examination, after the exhibition of a copious injection, I found a large quantity of the mucous membrane of the rectum protruded. It was highly vascular, somewhat thickened, and in a granular state. There were no distinct hemorrhoidal tumours. Around the prolapsus externally was a large quantity of thickened and relaxed integument, and the anal orifice itself was very capacious, and the sphincter was much weakened.

On examining him generally I found that he suffered, on slight exertion, from difficulty of breathing, and that the action of the heart was very irregular, the pulse being frequent, weak, and intermitting. He informed me that, not long since, he had suffered from œdema of the legs, and that his liver had been much enlarged, but that both these latter symptoms had become much decreased. It was hoped that, as his general health had been improved, the prolapsus would give less trouble, but the contrary took place, and accordingly Mr. Fereday submitted the case for my opinion as to how far, in such critical circumstances, any operative proceeding would be justifiable.

I informed the patient at once that the propalsus was so severe that the only mode of treatment which would effect a cure was the ligature, but that in his damaged condition I could not recommend it, as it would of necessity be attended with considerable danger to life, but that I would adopt a proceeding which would, in all probability, give great relief, and which would be unattended with danger. I, however, informed him that in such a bad case he must not expect the same cure as would follow the ligature. He determined to undergo the treatment proposed, and accordingly on the same evening I gave him an injection, and having got the prolapsed membrane well down, applied the strong nitric acid to it very carefully.

9th. He suffered pain for two or three hours after the acid had been

applied, but on evacuating the bowels this morning the pain was gone, and the protrusion was less. I therefore applied the acid freely again this evening.

11th. A severe attack of vomiting and retching—to which he has been subject—followed the application of the acid, but it has subsided, and the protrusion is less.

13th. This day I removed from around the anus the thickened folds of skin and mucous membrane, by making four separate incisions at right angles to the orifice, dipping the points of the scissors well inside the sphincter; a good deal of oozing of blood followed this operation, but it was arrested by returning the protruded membrane well within the sphincter.

16th. The bowels were opened to-day by castor oil, only a slight protrusion occurred, and it was readily replaced.

19th. The bowels having been well acted upon by castor oil, and the mucous membrane having been protruded, I again applied the nitric acid. I find the wounds made in the operation are nearly healed.

22nd. I applied the acid again.

24th. The bowels were operated upon naturally the first time this morning—the protrusion returned almost immediately by the pressure of the sponge: the wounds made by the scissors are quite healed.

26th. I applied the nitric acid for the fifth and last time to-day, after the bowels had been well acted upon by castor oil, the protruded membrane returned immediately with the least pressure, the sphincter ani contracts in a much more healthy manner. He is to return into the country in a day or two. I have desired him to take a dose of sulphur in milk at night in order to get a regular action of the bowels, to bathe the anus with the sulphate of iron lotion, and to pass a bougie every day after an evacuation.

In a month after his return, this gentleman wrote to me, "I have much cause to be thankful for the freedom I already enjoy from sufferings so long before endured," and two weeks afterwards he again wrote to me saying, "You will, I am sure, be glad to hear that since I last wrote to you I have several times walked from one to two miles without causing any return of my recent complaint;" and after referring to the annoyance still caused by his dyspnœa, he says, "I am, however, quite a new creature;" and with regard to the object aimed at by the treatment of bracing up the parts and getting the sphincter to a more healthy condition, the following remark shows its efficacy. It refers to the bougie, after the employment of which, he says, "The immediate contraction of the parts is so great that I have some trouble to get the bougie out again."

It is impossible, by any description, to give a fair picture of the distress which was caused to this poor gentleman by the constant and continued descent of the bowel, and therefore the relief of which he wrote in such thankful terms was especially gratifying and striking;

and I could not bring forward a case better calculated to illustrate the importance of having some means as a substitute for the ligature where this treatment could not be put in force without great danger to life. It is a maxim recognised, I believe, by all surgeons of experience, that when there is any organic disease existing either in the viscera of the abdomen, in those of the chest or in the brain, the operation of the ligature should not be performed; for whatever be the explanation of the matter, there is not a doubt that the stimulus of the operation sets up an increase of disease in the organs already affected, and may destroy life. A remarkable instance of this I mentioned in my last paper, and I think it very likely that had I applied the ligature in the case just detailed, some fresh disease would have been excited in the liver or heart, and a fatal result might have ensued. Had not we possessed any other means of remedying the very severe prolapsus, this patient must have lived in misery, and have been content with the use of mere palliatives which had already been exhausted upon him.

There are one or two details connected with the treatment of this case which it may be well to allude to more particularly. It will be seen that I applied the nitric acid both before and subsequent to the removal of the pendulous folds around the anus. I generally recommend the nitric acid to be used first, and the treatment to be finished by the employment of the scissors; but this gentleman, although he bore the treatment with courage and patience, had a great dread of that part of this proceeding, and therefore I hurried it on more quickly than I generally do. It is better to avoid using the nitric acid afterwards if possible, because the wounds made by the cutting instrument are likely to be disturbed and re-opened by the attempts of the patient to bring the bowel down and by the manipulations of the surgeon; but if there be a doubt about the matter, it is much better to apply the nitric acid on one or two occasions after the use of the scissors than to leave any of the protruded membrane thoroughly untouched. This lesson was forced upon me by a very unpleasant circumstance in my practice, where I treated a severe prolapsus on the same plan. I applied the nitric acid on one or two occasions before the loose folds were cut away, and as I thought sufficiently, and did not apply it afterwards. The consequence was, that in a few days after the treatment was, as I supposed, properly finished, some the symptoms returned nearly as badly as before, much to the annoyance both of my patient and myself. This accident, however, happened before I had had the experience of the treatment I possess now. If the nitric acid be applied with care, it gives but little pain and does no mischief. It therefore may be used on several occasions, if necessary, without rendering the treatment very severe.

There is one other point to which I wish to allude, and that is as to the use of the bougie after the treatment was finished. This is a most important matter to attend to, and should not be neglected

either after the use of the ligature or after the plan I adopt; its daily introduction after the evacuation of the bowels assists in replacing any portion of membrane which might be protruded, and the pressure of the instrument exerts a most beneficial action on the parts, preventing return of the disease. The use of a bougie is also beneficial in this way, it prevents the possibility of too great contraction of the anus occurring. This stricture might occur if the operation is not done with care, or if too much integument is cut away; and in my work on the rectum I have mentioned a case where an ill-judged and badly-executed operation was done by a Physician-Accoucheur, and was followed by severe contraction of the anus. Had a good-sized wax bougie been used in this instance for some time after the operation, this sad mishap would not have occurred.

I will now detail another instance showing the value of my mode of treatment where the ligature would have been out of the question.

I was requested by Dr. Tunaley, on January 6, to visit with him an old naval officer who was in his eightieth year, in very good health with the exception of a prolapsus of the rectum, with which he was confined to his bed. On examination I found a considerable tumour consisting externally of thickened and pendulous folds of skin, and internally of flaps of mucous membrane prolapsed in a very vascular state and not thickened. The history of this case was like that of many others: he had had the disease for many years, and had worn a very well made truss which generally kept the bowel up, but a few days before I saw him, as he was walking out, the bowel prolapsed by the side of the truss and he could not return it. Severe symptoms came on, he was treated by Dr. Tunaley for some days, and the parts having become more quiescent it became a question as to whether something effectual might be done. At such an advanced age it was of course out of the question to apply the ligature, but I stated to Dr. Tunaley my views about the combined operation with the nitric acid and scissors, and it was determined to put the treatment in force. Accordingly, the bowels having been previously well acted upon, I applied the nitric acid to one portion of the prolapsed membrane: the application produced scarcely any pain at the time or afterwards.

8th. The nitric acid was applied freely to the other portion of prolapsed membrane; this was not productive of any suffering.

10th. The acid having been found to have acted well in diminishing the prolapsus, I first benumbed the part with ice and salt, and then removed four flaps of the thickened integument from around the anus.

12th. Patient has only suffered very slightly. Wounds are healing well, and there has not been any prolapsus.

15th. Bowels have been well acted upon by castor-oil without the occurrence of any prolapse. The wounds have almost entirely healed.

I recommended him to take an electuary occasionally, and to pass a wax bougie three times a-week.

26th. I heard from Dr. Tunaley to-day to the effect that our patient was quite well, and had had no unfavourable symptom whatever.

I have narrated this case because it illustrates the value of the treatment adopted where a condition obtains, viz. that of extreme old age, which is generally considered by most surgeons as forbidding the employment of the ligature. Had there not been some other means at our disposal this old gentleman, who was for his time of life a remarkably healthy man, would have been compelled to suffer great discomfort for the rest of his days; for it will be seen that his truss had failed to give him that relief which it had done for some time. And I do not think that any surgeon would like to recommend the employment of the ligature in a person eighty years old, however free from disease his internal organs might be; but the combined proceeding of nitric acid and the use of the scissors is so free from danger that it may be recommended with the greatest confidence even in persons of very advanced age. It is true that sometimes it will happen that we are obliged to submit our patient to the ligature even when they are at a time of life when such a proceeding becomes very hazardous; but there are exceptional circumstances which render it necessary.

It will be seen that the instances detailed above were uncomplicated, though severe instances of prolapsus of the rectum, and about which there cannot be any doubt in the minds of those surgeons who have witnessed this treatment, as to the beneficial results which will ensue from it when undertaken with care. But cases will every now and then present themselves, in which the prolapsus is complicated with the existence of hemorrhoidal tumours, and, indeed, seems to be entirely produced by their mechanical weight. In such a case there is really some difficulty in knowing what to do when there is a contra-indication to the use of the ligature in the existence of some organic disease or old age. But I am glad to say that even in such a case I have found the proceeding adopted in the uncomplicated instances followed by very beneficial results.

A gentleman, aged seventy, consulted me on June 7. He informed me that he had suffered with piles and prolapsus for seven years, and that he had tried various remedies, but latterly the disease had increased, and become very inconvenient, in consequence of his not being able to return the bowel properly, so that, immediately he began to walk about, protrusion took place. On examination, I found that a protrusion was easily produced by the patient straining, and that it consisted of a large portion of the mucous membrane on either side, but slightly changed in its character. In addition to this, and just within the sphincter, were two or three distinct hemorrhoidal tumours, evidently of old standing. They were of a blue colour, chiefly venous,

and there was no tendency to bleed. The anal orifice was very capacious, and the integument around was loose and abundant. This gentleman would not hear of the ligature, so I told him that I would do my best to get rid of the prolapsus at all events, but that I was afraid I could not destroy the hemorrhoidal tumours. He readily acceded to submit to the treatment proposed, and accordingly, on the same evening, I applied the nitric acid to a portion of the prolapsed membrane.

June 11. The acid caused no pain, so I applied it to another portion of the diseased membrane.

15th. No suffering produced. He says the prolapsus is less, so I applied the acid again.

19th. I applied the acid again freely.

22nd. Prolapsus less. I this day excised from around the margins of the anus, three triangular folds of loose skin. There was no bleeding, and in the course of a few days the wounds healed up. He was for some little time a good deal annoyed by the hemorrhoidal tumours descending whilst the bowels were evacuated, and getting nipped by the now tightly contracting sphincter; but he wrote to me as follows, a month after his return into the country:—"The prolapsus seems cured, at all events it causes no inconvenience. The piles are frequently troublesome after much walking, but less so than formerly; and they are either considerably smaller, or at least they do not protrude so much. I am sure, sir, that you will be gratified to learn that your skilful treatment has been attended with results so far good, and as far as I can see, likely to be lasting."

Now, I must confess that this report was more favourable than I could have anticipated, for although I expected a diminution of the prolapsus and a more natural condition of the parts altogether, I did not think I should hear that the prolapsus seemed cured, for the hemorrhoidal tumours not being of that nature which would be remedied by nitric acid, and not daring to cut them off, I saw that they would probably hinder the good results of the treatment, but although my anticipations were to a certain extent verified, the words of the patient himself are the best evidence, that a very considerable amount of benefit had ensued from the remedial measures employed. The chief part of this benefit was, I doubt not, produced by the removal of the loose folds of skin around the anus, as the orifice was remarkably capacious.

It must be understood, however, that I do not recommend this plan of treatment in such cases of severe prolapsus attended with distinct hemorrhoidal tumours, for unless the latter are got rid of the prolapsus in general will persist; still, if the orifice of the anus is very capacious, and the integument around it is very relaxed, the operation of removing three or four transverse slips subsequent to the employment of the nitric acid will produce so much contraction of the parts, that the prolapsus may be almost entirely removed.

If space permitted, I could relate other cases where the combined treatment by nitric acid and subsequent removal of the loose tissue around the anus had been attended with excellent results; but I think it is not necessary to go into the particulars of other cases, and I will conclude my observations by a few directions as to the detail of the treatment, and the conditions under which it should be applied. And in the first place I must again distinctly state that my object is not in any way to discard the ligature in those cases where there is no contra-indication to that proceeding. I look upon it as most efficient, productive of remarkably little suffering and danger when properly performed, and giving much less trouble to the surgeon than the other plan; but when, from the causes mentioned, ligature cannot be adopted, the treatment in question may be put in force. In order, however, to obviate failure, which is very likely to occur, attention must be directed to one or two points. It will be useless to attempt to do any good with nitric acid in those cases where the prolapsed membrane has become much indurated and thickened, or where conjoined with the prolapsus, there are two or three large hemorrhoidal tumours of a blue colour, prominent and well defined. If the ligature is not admissible, or the patient will not submit to it, the best plan will be, in such a case, to trust to the effect of removing the folds around the anus, but the incisions must be more extensive, and half an inch or more of the diseased mucous membrane should be removed in at least four different directions, and as Mr. Syme tells us, this operation alone may be attended with effectual relief, but when the mucous membrane is very voluminous, and the muscular tissue of the bowel is prolapsed as well, failure is likely to follow this method. In such a case the only thing to suggest is, that the patient should wear a well-made pessary, which will give great comfort.

When the mucous membrane is not indurated, is highly vascular, and readily bleeds, however voluminous it may be, the nitric acid, if sufficiently and carefully used, will almost unfailingly do good, sometimes the propalsus will quite disappear; whilst in others the only symptom of benefit is the readiness with which the part is reduced by the patient after the bowels have been evacuated. When this is the case, we may be certain that the remedy is acting well, and it should be repeated until the whole diseased membrane is touched.

The second part of the operation, consisting in the removal of the loose folds around the anus, requires great care and nicety. It should be practised a few days after the last application of the acid in the following manner:—Whilst the patient is kneeling on a chair or lying on the side of the bed, whichever gets the best light, the surgeon seizes a fold of the loose integument with either a pair of broad-bladed dressing forceps or an ordinary hooked forceps, about half an inch from the anus, and holding the fold well up with his left hand he takes a very sharp pair of scissors, curved on the flat, and beginning his incision from the outside, excises the part, carrying the point of

the scissors well into the anus so as to remove portions of mucous membrane as well as skin—thus the incision is made transversely and not longitudinally, as recommended by Mr. Curling and Mr. Quain. I generally make three or four such incisions. The effect of this is to produce a healthy and permanent contraction of the anus and lower part of the rectum, so that the good result already produced by the nitric acid, and which is not sufficient of itself to remedy the disease when severe, will be greatly enhanced, and nearly as good a cure will follow as if the ligature had been used. What will not take place if only one of the measures is adopted, will be produced by the combination of the two methods—just as in like manner we find that one drug, however powerful, will fail to produce any good effect, whilst a combination of two or more possessing similar qualities will accomplish the object desired.—*Med. Times and Gaz., Sept.* 14, 1861, *p.* 266.

60.—ON FLAT ULCERS OF THE RECTUM.

By William Coulson, Esq., Surgeon to St. Mary's Hospital.

Ulcers and fissures of the rectum are amongst the most painful of the minor affections with which surgery is concerned. Where, however, any difficulty exists in such cases, it usually arises in the diagnosis rather than in the treatment. Commonly, when the seat of fissure is discovered, the division of the fibres of the sphincter by the knife offers a ready means of cure. In cases of flat ulcer, however, the same result may be obtained by milder means, as in a case treated this year by Mr. Coulson at St. Mary's Hospital. The patient was a man aged 26 years. He had been the subject of piles for some years, which he attributed to an early habit of taking strong purgative medicines. Three weeks before admission he had felt a violent throbbing in and after action of the bowels; this was followed by the discharge of about an ounce of blood and continued tenesmus. These symptoms were renewed at each action of the bowels, until admission, the pain being severe, and lasting some hours. On examination, a somewhat deep and irregular ulcer was found by Mr. Coulson to exist in the mucous membrane within the sphincter. It was ordered that the bowels should be regulated and maintained in a condition of laxity by determinate doses of castor oil, that careful precautions for cleanliness should be observed, and that an ointment should be applied composed of a drachm of the nitric oxide of mercury to an ounce of lard. Under this simple treatment the patient made a rapid recovery, and was discharged, cured, at the end of a fortnight.—*Lancet, August* 17, 1861, *p.* 160.

61.—ON ABDOMINAL OR PELVIC ABSCESS.

By Frederick C. Skey, Esq., F.R.S., Surgeon to St. Bartholomew's.

[Probably all abscesses on a large scale, are rather the result of low, than of an exalted vitality. Accordingly, pelvic abscess commonly

occurs in persons of impaired constitutions, reduced by low living, accidental illness, or any debilitating cause. The general situation in which pelvic abscess occurs, is the iliac fossa, but it is situated within the general walls of the abdomen.]

It appears in the form of a firm, not necessarily a hard, swelling, very distinctly perceptible on pressure over the above region. If small, its presence is only readily detectable by comparison with the opposite fossa, into which the ends of the fingers sink on moderate pressure. In this respect, however, there is a difference appertaining to the varieties in the form and quantity of the contents contained within the abdominal walls, especially of fat, and the greater or less laxity of the walls themselves. Occurring in young women shortly after parturition, its presence is remarkably distinct. When large, the swelling is apparent to the eye as well as to the touch, and it extends across the abdomen towards the mesial line, and upwards in the direction of the umbilicus. In such cases the swelling is prominent, and as it increases in magnitude, it encroaches on the intestines, which are pushed across to the opposite side. In many cases pain is not a prominent feature, and when present, it is usually not severe, but is dull and aching rather than acute in character.

In its early stage I have known this form of disease to be mistaken for two other varieties of swelling—malignant disease of the pelvis, and scybalæ in the colon. From malignant disease it may be distinguished by the general uniformity of the swelling, and by the less serious constitutional signs of health undermined; and judging from liabilities, malignant growths of or from the pelvis are far more uncommon than chronic abscesses. These examples of pelvic abscess have presented themselves to my observation more commonly on the right than on the left side. I am not aware whether scybalæ collect more usually on that than on the left; but certainly they are more palpable and more readily detectable in the head of the colon than in the descending part of the intestine, which is placed in less proximity to the abdominal walls than that on right side. But scybalæ are limited in their relation to the front abdominal walls by the caput coli, and moreover are movable, whereas the mass, which gradually resolves itself into the abscess, presents to the hand the sensation of a large and solid deposit, firmly fixed, and considerably larger than the intestine itself. The disease progresses very slowly, and often requires weeks for its development. As a general rule, it becomes soft, or, in other words, is converted into an abscess, which occupies its original site; in other cases the sac yields, and the matter extends in one or more directions, of which the most common is downwards along the track of the femoral vessels. I have seen three examples in which the matter made its way backwards through the sacro-ischiatic foramen, presenting the dimensions of a formidable abscess on the buttock, the upper part of the thigh, or over the region of the trochanter major. Occasionally the matter will extend across the

abdomen behind the peritoneum, and take the course of the vessels of the opposite side, where it may point through the abdominal muscles above Poupart's ligament. In the case of a lady to which my attention was recently called, a large collection of matter formed underneath the integuments of the right iliac region. The abscess had been preceded by a large mass occupying the fossa beneath it for a period of many weeks. After having evacuated the sac, I detected an opening through the aponeurosis of the external oblique muscle sufficiently large to admit the end of the forefinger, through which the matter had escaped from the abscess below. The outer cyst contained about a pint of brownish pus, both in colour and consistence very unlike the fluid contained in a psoas abscess.

Two remarkable examples of this disease have been under my treatment in St. Bartholomew's Hospital within the last few months. The first was that of a boy about fifteen years of age, who fell down on the ice in January last, and struck his left trochanter severely. Considerable pain followed the injury, which was deemed rheumatic; and he was admitted into the above hospital and placed in a medical ward. While there, a swelling formed over the *right* iliac fossa, for which he was removed into a surgical ward, and came under my care. He had an abscess pointing both above and below Poupart's ligament. The upper projection was very prominent, and threatening shortly to burst its way through the skin; but I preferred to evacuate the abscess through the lower opening. About six or eight ounces of matter escaped. For some days he appeared much relieved by the operation; but he gradually lost his appetite, became hectic, and died in three weeks. On examination, it was discovered that the fall had occasioned a rupture of the fibrous capsule of the left hip-joint, and fracture of the acetabulum. Matter had formed in this region, which had extended across the abdomen behind the peritoneum to the right side, and made its way through the abdominal parietes where I had punctured it. During life the boy did not complain of any pain or even discomfort on the left side; and what is worthy of remark is, that during the last week of his life he lay over entirely on his left hip.

When these pelvic abscesses are large and their progress towards maturity is unusually slow,—when they are placed deeply within the pelvis and the matter is bound down by the pelvic fascia, they seldom fail to involve contiguous bone; in which case they generally prove fatal.

All that we can hope for, all that the best recources of art can achieve, is to change the chronic into an acute abscess, to advance the formation of pus, and to compel the abscess to select that locality through which it can most readily discharge its contents on a surface of the body. To effect this the appetite must be improved, and gratified with as large a quantity of nutritious food as can be digested; force and vigour must be given to the pulse by means of stimulants, —and the capacity for stimulants in these cases of debility is very

great,—while the lungs should be supplied with an ample quantity of fresh air for the thorough oxygenation of the blood.

If there be one therapeutic agent more valuable than another in promoting suppurative action, it is bark, and it should be given throughout the treatment in full quantities. At the earliest moment at which fluid can be detected near the surface the abscess should be freely opened. It commonly points through the abdominal muscles, but the rule equally applies should the abscess point towards the rectum, or, when occurring in the female, towards the vagina, or on the nates or region of the trochanter.—*Lancet, April* 27, 1861, *p.* 406.

ORGANS OF URINE AND GENERATION.

62.—ON INCISIONS FOR STRICTURE COMPARED WITH DILATATION.

By John Gay, Esq., Surgeon to the Great Northern Hospital, &c.

I believe with Mr. Syme, that a stricture, not of traumatic orgin, rarely if ever becomes impenetrable to a skilfully plied bougie or catheter (I invariably use the catheter); and, moreover, that when once passed, the catheter, if its use be steadily and cautiously persisted in, and combined with appropriate general and local treatment, such as leeching, opium, steel, &c., will ensure as complete a cure as can be effected by any other known means.

The advantage of this method over external division is its comparative freedom from risk; and I cannot but regard the argument in its favour as irresistible when we come to reflect that, out of every hundred, according to received tables, on whom division has been practised, six individuals have perished. This rate of mortality would be insignificant if the disease itself were mortal; which is not the case at that period of life in which such a procedure could for a moment be justified; or, perhaps, if there were an equivalent in the number of actual cures. It is true, that strictures do occasionally reach such a pass as to lead their sufferers to "protest that life is no longer desirable"; still, I cannot think that such a protestation, independently of some killing circumstances about the disorder itself, becomes, in any case, an admissible plea for an experiment in the shape of a possibly fatal operation.

It may have been observed that I do not exclude external division from the list of stricture remedies, believing that the real aim of the surgeon should be to "utilise all"; but I do object to that estimate of the operation according to which it is regarded as a simple, safe, and effectual method of cure; and is, in consequence, adopted, as I have much reason to believe it still is by some, without either that

regard to necessity, or that amount of caution which should ever be observed when life hangs on the issue.

Of the simple internal incision recommended by Mr. Thompson, I have had no experience; but, when confined to the constricting band or tissue, I can see no objection to it; believing that whilst, on the one hand, it can do no harm, it is very likely, on the other, to relieve congestion, inflammation, or irritability, and efficiently to prepare the way for what, in the end, will still be the "wedge" in the treatment, namely, persistent dilatation.

I come now to speak of dilatation, having long adopted Sir Chas. Bell's view that "the permanent cure of stricture is to be accomplished by dilatation.....by such a stretching of the narrow part of the canal, that it grows large under the operation, and has no disposition to contract." ('Institutes of Surgery'.)

I speak from some considerable experience, and that of the treatment of the worst forms of stricture, when I say that I have very rarely met with a case that has not been amenable to the use of the catheter, with such adjuvants as opium, leeches, steel, &c., when especial circumstances demand them.

Space forbids my treating of different kinds of strictures in detail, or their pathology; but it must be remembered that that idea of an urethral stricture is miserably imperfect, which reduces it to the simple narrowing of the passage. A stricture is a structural lesion; and is not only the consequence of some disorder of the urethral tissues, but becomes itself the centre, and, perhaps, the seat of disorders specifically its own, such as inflammation, congestion, hemorrhage, spasm, &c. And it is most constantly the case that the patients suffering from stricture do not become conscious of its existence, or at least do not awake to its importance until their attention is drawn to it by its having called into painful activity one or the other of these accessory forms of disease.

It is not, then, the stricture *per se*, except in urgent cases, that should first claim the surgeon's notice, but its incidental affections: and all mechanical treatment should be withheld until these are brought under control by appropriate management, or, if the delay can be borne, altogether overcome.

The most general causes of failure in the use of the catheter are, as far as I have seen:—1. Its inopportune employment; and 2. Impatience. To the first I have just alluded. With regard to the second, I should say that in no case should the catheter be used with any but the slightest force; and this should not be persisted in but for a very short time. It is better to make several efforts at intervals of one, two, or three days, than overcome the resistance of a stricture by one effort, if that should necessarily be a violent one, and such as would lacerate or do other injury to the urethra. Gentle and regularly repeated efforts will eventually ensure success in, I believe, the most aggravated cases. I am in the habit of observing two rules when at-

tempting to pass a catheter through a stricture, of the greatest service: 1. Invariably to keep the convexity of that part of the catheter between the curve and the point well back against the posterior wall of the urethra, the point being tilted slightly forwards and upward at the same time by a finger commanding the handle side of its curve; bearing in mind the anatomical relation of the urethra to the pubic arch, as it passes through the triangular ligament; and 2. To desire the patient at the same time to make an effort to void his urine, whereby all resistance to the introduction of an instrument, on the part of the extrusor or detrusor muscles of the urethra, is overcome, and a channel, though ever so small, is, of necessity, formed.

In passing, I might remark that this is an invaluable test of the *existence* of stricture, especially in young subjects.

After the catheter has once passed through the stricture, it does not follow that it will be admitted on subsequent attempts. These may fail from many causes, and may fail even again and again; but, after a time, the failures will be less signal, until, at length, they cease altogether.

There should never be a hurry to substitute a larger instrument for one that passes. Sometimes a small instrument will dilate, and that with permanency, more quickly than a large one; although, on many accounts, the latter is preferable to the former in the generality of cases. I am assured that, by the gentle but patient and persistent use of catheters, well adjusted, as far as their size and shape are concerned, to the capacity of the strictured passage, and to the normal or abnormal course of the urethra in its entire length, most strictures can be overcome, and the urethra restored to its original condition, with as much security against relapse as by any other plan of treatment that has hitherto been devised, and with less risk of injury to the patient; and that the call for the knife is exceedingly exceptional. —*Brit. Med. Journal*, *June* 8, 1861, *p.* 603.

63.—ON THE IMMEDIATE TREATMENT OF STRICTURE OF THE URETHRA.

By Barnard Holt, Esq., Senior Surgeon and Lecturer on Surgery at the Westminster Hospital.

[At first sight we should be afraid of the practice of Mr. Holt in *splitting up* a stricture by force, but the following paper and cases are important, and deserve serious consideration. He says:]

My attention was early attracted to the prevailing defects in the treatment of strictures. I could not but observe the tediousness of the treatment by ordinary dilatation, occupying many months before an average instrument could be introduced into the bladder, and that even when the dilatation was accomplished, the constriction generally returned, so that perpetual surgical care was required.

Being deeply impressed with the unsatisfactory nature of the prevailing methods of curing these distressing maladies, about seven years ago I adopted a more energetic mode of treatment, and invited the notice of the profession to a new "Stricture Dilator." Its use was at first limited to simple dilatation, which was readily effected by graduated tubes passed between the blades without the withdrawal of the original instrument. Experience, however, soon showed me that, as a general rule, when dilatation was carried much beyond the degree produced by ordinary bougies, "stricture fever" was induced. I therefore determined, though with some apprehension as to the consequences, *to split the stricture* by passing the largest sized tube at once, and thus immediately to enlarge the contracted part of the canal, so that it might receive a catheter equal to the entire breadth of the urethra.

Fearing the effects of the urine being permitted to come in contact with the laceration thus occasioned, I kept a gum elastic catheter in the bladder, but as this measure gave rise to considerable irritation, I determined to content myself with simply splitting the stricture, drawing off the urine, and not again using the catheter till two days after the operation. After that interval an instrument of the same diameter as that used at the time of the operation was again employed, and its use was continued, first on alternate days, and subsequently at longer periods of delay.

The course of experience has shown, indeed, that instances occur in which it is necessary to use a catheter one size less than that first passed after the operation, but such cases are exceptions. The instrument by which this simple process is accomplished consists of two grooved blades fixed in a divided handle, and containing between them a wire welded to their points, and on this wire a tube (which, when introduced between the blades, corresponds to the natural capacity of the urethra) is quickly passed, and thus ruptures or splits the obstruction. The simplicity of this apparatus is obvious to all, and the history of above a hundred cases proves that its use is unattended with any of those serious complications,—viz., hemorrhages, false passages, infiltration of urine, perineal abscess, fistulæ, swelled testis, &c., &c.—which too often accompany the other operative process devised for the relief of these organs. The forcible distension caused by the dilator affects the morbid obstruction only; the healthy portion of the canal is not disturbed, the slightly vascular character of the strictural deposition and resisting tissue gives rise to but very inconsiderable bleeding, and the subjacent adventitious texture perfectly obviates infiltration into the surrounding cells. A perusal of the following cases, which are extracted as salient examples from a long series which were witnessed through their whole career by the students and surgeons who favour the practice of the Westminster Hospital by their attendance, will, I think, corroborate the points which I wish to

establish, and I hope justify me in upholding as proved the following conclusions :—

1. The operation is of the most simple kind, and any one who can pass a bougie through a difficult stricture is competent to perform it.

2. It is not attended with hemorrhage, infiltration of urine, abscess, or any serious local mischief.

3. In the majority of instances the relief is immediate.

4. The occurrence of rigors or any other constitutional disturbance is very rare, and the patient is seldom confined to bed longer than from twelve to twenty-four hours.

5. The urethra is immediately made permeable by a catheter of full size, which may be ever afterwards passed at discretion.

6. This method is available in every kind of stricture where a canula of any size can reach the bladder.

7. When the after treatment is judicious and attentive, the full capacity of the passage is always maintained.

8. In all cases of neglected treatment the stricture yields to this method more promptly than to any other.

9. It being impossible that any but the diseased tissue can be divided, the splitting of the stricture has a decided superiority over any cutting operation.

10. And to sum up the great advantages in one proposition, the process is facile, speedy, prompt in effects, and free from every danger, immediate or remote. The course of general treatment will naturally vary, according to the kind of obstruction, the number of strictures, and the occasional complications of contracted bladder, enlarged prostate, fistulæ in perineo, false passage, &c. In simple stricture, however narrow, the relief will be immediate, but in the more complex forms of these maladies, the size of the stream is not so directly increased as might have been anticipated, from the immediate enlargement of the canal. Notwithstanding, however, the stream may for a short time remain somewhat restricted, the patient is able to empty his bladder much more quickly and effectually, and has less frequent micturition. The limitation of the jet evidently depends upon the thickening of the surrounding textures, and swelling of the mucous lining; these morbid states speedily subside, and in a short space of time the patient can void his water in a normal manner.

Case 1.—*Stricture of Twenty-five Years' Duration—Severe Constitutional Disturbance—Recurrence of "Stricture Fever" after each attempt to introduce a Catheter—Retention of Urine—Hemorrhage—False Passage—Operation—Recovery.*—Thomas W., aged 50, a labourer, of dissolute habits, was admitted under my care, on Nov. 5, 1857. He had been the subject of stricture for twenty-five years, during which period the stream has been gradually contracting, and the urine is now passed guttatim. Five years since he was an in-patient of St. Thomas's Hospital, where, after considerable difficulty, a No. 1 catheter was introduced and retained, being replaced by

others in succession until No. 6 could be passed, when he left. The contraction speedily recurred, and he was admitted into the Westminster Hospital as above. He is now greatly emaciated, with brown and dry tongue; pulse 110; countenance pallid; appetite defective; abdomen tympanitic; nights restless; and there is a constant escape of fæces during the straining to evacuate the bladder, an effort which he is compelled to repeat every hour and a-half. The house-surgeon having twice failed in passing a No. 1 catheter, each attempt being followed by syncope and subsequent rigors, I saw the patient, and directed the nightly use of the warm bath, and prescribed salines with opium through the day, and castor-oil every morning. At the expiration of a week his general condition was sufficiently improved to justify the attempt to introduce a No. 1 catheter, when a stricture was detected five inches from the meatus, through which it was impossible to penetrate. Although the examination was conducted with the greatest gentleness, it was followed by a severe attack of "stricture fever," which was only relieved by the administration of opium. The urethra being irritable, and the patient much exhausted, another week was permitted to elapse, during which time quinine and stimuli were administered, but the second trial was likewise futile, and followed by precisely the same results as the first. After the lapse of a third week, and while the patient was fully under the influence of opium, a third attempt was made, but with no better success. I therefore determined to place the patient thoroughly under the influence of chloroform, when, after very considerable difficulty, a No. 1 catheter was introduced, and a pint of highly-offensive and turbid urine withdrawn. In the evening he had retention which, after great difficulty, was relieved by a No. 1 gum catheter; there was considerable hemorrhage, the patient losing from ten to twelve ounces of blood. He passed a restless night, and being unable to make water, the catheter was again had recourse to. (The house-surgeon had unfortunately omitted to retain the former). As it was found to be impracticable to re-introduce it, the man was again anæsthesiated, and another attempt made, but although the catheter passed its whole length no urine followed, and it was evident a false passage had been made. Opium and the warm bath were had recourse to, and in a few hours a small quantity of bloody urine escaped and continued to dribble away during the day and night.

When I saw the patient on the following morning his tongue was dry and brown; pulse feeble and rapid; skin hot, dry, and blanched; and he complained of great tenderness upon pressure over the lower part of the abdomen; he was immediately ordered calomel and opium every four hours, and a turpentine fomentation over the seat of pain. For the next few days he continued in a very precarious state, but the symptoms gradually yielded, and in ten days from the retention the urine was free from blood; his health was, however, so much shattered by the accompaniments of the retention, that a month

elapsed before any further instrumental attempt was made. Former experience having shown the impossibility of introducing any instrument excepting while under chloroform, he was once more anæsthesiated, and the dilator with difficulty passed into the bladder; the large sized tube was then quickly passed, the stricture fairly split, and the dilator removed. A No. 12 catheter was next passed, and the urine withdrawn; the bleeding was very trifling. Ordered a mixture of quinine and opium, two grains of the former and ten drops of the latter for a dose. In the evening he felt chilly and uncomfortable, but had no shivering; the urine flowed with moderate ease, and was triflingly tinged with blood.

On the following day he was able to walk about the ward, the urine came away in a small but clear stream, the medicine was omitted, and his ordinary diet resumed.

On the second day after the operation, a No. 12 catheter entered the bladder without the least difficulty, the urethra was tender, but he did not experience so much pain as on former occasions, the after treatment was continued at increasing intervals, and he shortly left the hospital capable of passing his own instrument.

The foregoing is a case of very considerable interest, as embracing many of the most important points in connexion with stricture. The duration of the disease, the manner in which micturition was accomplished, the deplorable state of the patient's health, the supervention of "stricture fever" after every attempt at catheterism, the after establishment of false passage, and the profuse hemorrhage, are all evidences of the serious nature of the case, and consequently presented a severe test to the adopted treatment, and the perusal has, I hope, satisfactorily shown immunity from fatal consequences, rapidity of recovery, and complete restoration of the urethra to its natural size.

Case 2.—Stricture of Fourteen Years' Standing—of a Dense Cartilaginous Character—Situated about the Triangular Ligament—Urine, which was Ammoniacal and Purulent. passed Guttatim—Operation—Recovery.—John W., aged 45, a labourer, was admitted in December, suffering from stricture of the urethra, of fourteen years' duration. Seven years since, the urine having been for some weeks previously passed guttatim, he had retention, and applied at St. George's Hospital, where after considerable difficulty a catheter was introduced. He continued an in-patient until No. 4 could be passed, and then left, much improved both in health and manner of micturition. The stricture, however, gradually returned, and after several attacks of retention, and a generally increasing difficulty in relieving himself, he became an in-patient at the Westminster Hospital. At the time of his admission, the urine was passed every hour, with great straining, and frequently in drops, there was considerable hardness in the perineum, and his health was much damaged, as evinced by emaciation, pallor, loss of appetite, sleepless nights, and general

feverishness, the urine was highly ammoniacal, and loaded with mucus and pus. A No. 1 silver catheter was attempted without success, there was complete obstruction at the triangular ligament, and although the point was firmly grasped, it could not be made to penetrate through the stricture. The patient was kept in bed and the bowels regulated, and on the third day from his admission, another attempt was made in the most gentle manner, but with no better result. Considerable irritative fever followed the two trials, and it was only after the expiration of two months, during which time six endeavours were made, that a No. 1 catheter was passed. The stricture was of the cartilaginous variety, about an inch in length, and gave that peculiar grating to the catheter so specially characteristic of cartilaginous obstruction. A large quantity of highly-offensive urine was withdrawn, and the catheter was retained. On the following day, a larger size was substituted, and on March 4, the patient having been placed under the influence of chloroform, the dilator was with considerable difficulty introduced, and the No. 12 tube immediately passed. A No. 12 catheter was then easily slipped in, and the urine withdrawn. On the following day there was no febrile disturbance, and the patient declared he made water better than he had done for many years. On the succeeding. or second day after the operation, the urine was passed in a smaller stream, and with some scalding, consequently a No. 11 catheter was used, the urine was much less fetid and less in quantity than before. The stream continued small for a fortnight, but the urine was expelled in a much shorter time, and the frequency of micturition had materially diminished, he now only required to relieve himself three times during the night, and the same during the day. The No. 11 having been passed on alternate days, a No. 12 was now taken, and used every third and fourth day in the manner already described. Six weeks after the operation he left the hospital, making water in a perfectly natural manner, having been taught to pass a No. 12 with ease.

The foregoing, one of a class that offers the greatest impediment to the introduction of a catheter, is an excellent example of a formidable cartilaginous obstruction situated at a part of the urethra which, corresponding to its curve, gives greater trouble in the introduction of an instrument than any other. The patient had been the subject of stricture for many years, his urine had for months previous to his admission been passed guttatim, his clothes were constantly saturated, and his health was materially damaged by the hourly necessity for relief—an evil which depended upon the stricture, as proved by the large quantity of urine that was withdrawn when a catheter was introduced. All these difficulties were immediately overcome, and a large-sized catheter could be ever afterwards introduced with perfect facility. The offensive ammoniacal condition of the urine gradually abated, that fluid assumed a normal character, and the bladder being no longer irritated by its detention, the intervals between the time of

micturition were prolonged. In a word, the patient was restored to complete comfort without having been detained in bed more than a day, and without experiencing a single unfavourable symptom, indeed, he did not suffer in the same ratio as during the attempt to simply introduce a catheter. To those who are uninitiated in the treatment of stricture, it might appear that by keeping a catheter in the bladder, and daily increasing the sizes, the same results would have been obtained, but experience proves this plan of treatment to be utterly futile, and that as soon as the catheter is removed, so soon does the stricture return.

Case 3.—Stricture at the Bulb of Twenty-eight Years' Standing —Frequent Attacks of Retention of Urine—Infiltration—Fistulæ in Perineo and Scrotum—Enlargement of the Prostate Gland.—H. R., a captain in the Navy, aged 58, consulted me after having for many years been suffering from the effects of stricture. He had had repeated attacks of retention of urine, and upon three occasions matter formed behind and in the scrotum, which eventually gave rise to fistula in those situations. At the time of his coming under my care, the greater portion of the urine escaped through these apertures, so that he was always compelled to make water in a sitting posture, or with the aid of an earthenware slipper held between the legs. His clothes were constantly saturated with these dribblings, and his health was materially damaged by constant straining, and the frequency with which he was compelled to make efforts to relieve the bladder. He had been under the care of many surgeons, both in London and elsewhere, but without obtaining amelioration to his sufferings. Having tried various sizes and kinds of instruments, I was fortunate enough, at the expiration of three weeks, to get a 0 0 silver catheter through the obstruction. This instrument was retained, and replaced by a larger size, until No. 3 could be passed, when the dilator was introduced, and the No. 12 tube immediately passed. The same after treatment was adopted as in the preceding cases, and in three months from his first visit he was enabled to pass No. 12 without difficulty. The stream was good, the intervals much longer, and his health better than it had been for the previous seven years.

Another very similar case may be added, viz:—A gentleman, a solicitor by profession, was affected with a stricture of many years' standing. He had for ten years experienced great difficulty in emptying his bladder, and the urine was loaded with mucus. Upon the first examination the catheter was arrested at three inches from the meatus; after some trouble it passed on to a second obstruction, near the membranous portion of the urethra, through which it was impossible at the time to penetrate. Upon subsequent trials, however, I got through the second obstruction, and came upon a third, nearer the bladder, which was eventually overcome. The dilator was at once introduced, and the three strictures split. A No. 12 catheter was immediately pressed in, and the urine withdrawn. Considerable pain

was for a time experienced in this instance in expelling the urine, which was only effected with great straining. The cause, however, of this suffering was speedily apparent, for the patient passed three small calculi, which had formed behind each of the obstructions, and were washed away when the urethra became enlarged.

[Mr. Holt, in another number of the 'Medical Times and Gazette,' concludes this interesting series of cases, and ends his paper by the following remarks :]

The foregoing cases have been briefly described, and no allusion or comparison has been made to the various kinds of treatment already published. Their perusal must have satisfied the most sceptical that in every instance where any kind of instrument can be passed into the bladder the urethra may, by the mechanical effect of the dilator, be immediately enlarged to its natural size ; and that while in slight cases this enlargement is effected without suffering, or even the inhalation of chloroform, in the more severe forms that agent facilitates the operation and secures a perfect immunity from pain. It is quite true that if the after-treatment is not attended to the stricture will sooner or later recur, but as the patient by passing his own instrument has the control of the bladder, it can only return as a consequence of culpable neglect. The immunity from accidents having been already proved, the surgeon need have no fear of those serious results which so frequently accompany any cutting operation, and it is no less extraordinary than true that whereas rigors ordinarily supervene after rapid dilatation, they form the exception where the stricture has been fairly split.

The method of performing this operation may be described in a very few words. The permeability of the canal having been once satisfactorily ascertained, the size of the meatus of the urethra is to be gauged by passing into it a sound that will conveniently fit, and the number of the sound so used is to be the number of the tube to be passed : this it is important to ascertain, so that the urethra may not be stretched beyond its natural limits, for while the urethra of one person will admit a No. 14, another will not admit more than No. 9.

The dilator having been previously well oiled, is to be introduced with the handle somewhat over the patient's left hip, and by keeping the convex portion gently pressing against the under part of the urethra, the point will glide along the upper portion until it is fairly beyond the triangular ligament, when, by bringing the handle to a right angle with the body, and gradually depressing it—but not so much as in the passage of an ordinary catheter—it will usually slip into the bladder; in fact, the same proceeding is to be adopted as in introducing a lithotrite for the purpose of crushing a calculus. Having reached the bladder, the dilator should be gently rotated, to prove that it is fairly within that viscus, and being thus assured, the surgeon is now to place the point of the tube he had previously selected upon

the wire between the blades, and thrust it quickly onwards to the end. The stricture being now fairly split, the dilator should be rotated to still further separate the tops of the rent, and be then withdrawn. A catheter corresponding to the number of the tube being substituted for the purpose of removing the urine. The catheter is now to be taken out, and the patient sent to bed, with directions to take, every four hours, for the first day and night, a mixture containing in each dose two grains of quinine and ten minims of the tincture of opium. The facility with which this proceeding can be effected will of course depend upon the kind and number of the strictures, and the existence or otherwise of false passages or fistulæ in perineo. The urine being withdrawn, the patient does not require to pass water for some hours, and when compelled to do so, the stream is usually larger, and the urine passes with greater facility than before. On the second day from the operation the same catheter should be gently introduced; but if the patient complains of much scalding, it will be better to take one size less. This should be repeated every other day for a week, when the larger one may be substituted, and the patient be taught to pass his own instrument. Of course the time occupied in the after treatment must vary with the nature of the case, and in the more obstinate forms necessitates the employment of the catheter for some time, the intervals being gradually increased until it is not required to be used more than once in three, four, or six months, and in most instances not more frequently than once a year. The bowels should be relieved by a dose of castor-oil taken early on the morning of the operation, and the patient should be directed not to pass water for two or three hours previously, in order, 1st, to facilitate the introduction of the dilator, and 2nd, to permit its free movement in the bladder.

Out of so large a number of cases in which this instrument has been employed, instances have occasionally occurred where the after treatment has been greatly neglected, and the patients have presented themselves again. In these cases it has always been found that the calibre of the urethra may soon be restored by the dilating power of the instrument, without again resorting to splitting, the uniting medium yielding much more easily than the structures which formed the original stricture.

In conclusion, it may be stated that the dilator is not only capable of splitting a stricture, but it affords the most effectual means of dilating it to any extent that may seem advisable to a prudent surgeon. All practical surgeons know the difficulty of passing a second catheter through a difficult stricture. This is entirely obviated by the use of the dilator, which, being once introduced, is capable of either dilating the obstruction by the employment of a succession of tubes, or splitting the stricture by the passage of the largest tube at once.—*Med. Times and Gazette, Oct.* 19 *and* 26, 1861, *pp.* 398, 428.

64.—TREATMENT OF STRICTURE OF THE URETHRA BY SUDDEN AND FORCIBLE DILATATION.

By Christopher Heath, Esq., Surgeon to the West London Hospital, and the St. George's and St. James's Dispensary, &c.

[Mr. Heath confirms Mr. Holt's mode of treating strictures of the urethra, as illustrated in the preceding article, and gives the following interesting cases. He says:]

Whether a stricture can ever be said to be absolutely *cured* must remain an open question; but a method of treatment which so rapidly restores the urethra to its normal size, and allows of the patient's continuing in perfect health for many months afterwards, as in these cases, is certainly worthy of trial.

Cases 1 and 4 were strictures caused by repeated gonorrhœas, and were of the densest character, the latter being complicated by a *fistula in perineo*. Cases 2 and 3 were of a more intractable nature, being of traumatic origin, the tendency to recontraction being one of the great characteristics of the affection. In Case 2, the perineum had been perfectly riddled with fistulæ, which had converted it into an irregular, hardened mass, such as is seldom witnessed, which, however, rapidly disappeared upon the urethra being restored to its normal condition.

It will be noticed that in none of these cases was there any "urethral fever" after the operation, and in one only was there a rigor, which took place on the day *after* the operation, and was consequent upon the passage of an instrument, which might as well have been omitted for a day or two.

Case 1.—Mr. E. P., aged 25, came under my care in January, 1860. Has lived a very fast life, and had gonorrhœa repeatedly. Four years since after a long gleet, he first suffered from retention, which was relieved by the use of a catheter; but he underwent no regular treatment for the stricture, and since that time he has suffered occasionally from retention, and the stream has been decreasing in size. Two years ago a surgeon passed No. 4 with great difficulty to relieve retention; but this gave him such pain that he has never had an instrument passed since. He has been living carefully for the last year, and his general health has been pretty good, although he is thin and pale. He suffers from frequent nocturnal erections and emissions; the stream of urine is very small and twisted, and is passed with great difficulty and straining.

The patient having the greatest dread of pain, on the 14th of January, 1860, chloroform having been administered, I proceeded to examine the urethra. Found a very dense cartilaginous stricture (which could be clearly felt from without), about two inches from the orifice, and through this with very considerable difficulty I introduced No. 00 silver catheter (*i.e.*, two sizes smaller than the ordinary No. 1). Another stricture existed at the bulb, which gave some trouble; but

this was at length overcome. and the catheter entered the bladder. Instrument tied in. Ordered half a drachm of tincture of opium.

Jan. 15th. Tried to replace No. 00 by No. 1, but failed (without chloroform), and therefore replaced No. 00. Has had no shivering nor inconvenience, except the presence of the catheter.

16th. Introduced No. 1, and tied in.

17th. Passed No. 2, and tied in.

18th. No. 3 tied in. Lies on the sofa, and suffers little inconvenience.

19th. No. 4.

21st. Chloroform having been again administered, I passed Mr. Holt's dilator (small size) through both strictures into the bladder, and then introduced No. 4 tube. Having withdrawn this instrument, I was then enabled to pass the full-sized dilator while closed, and into it I at once introduced No. 10 tube, which required some considerable force, owing to the density of the stricture. The dilator having been withdrawn, a No. 10 silver catheter passed readily enough; the bladder was evacuated, and the instrument withdrawn. To take half a drachm of the tincture of opium.

22nd. Passed a very good night; has a little smarting when the urine is voided. Passed No. 10.

23rd. No. 10 passed readily.

25th. Walked to my house, where I introduced No. 10.

Feb. 1st. Has been prevented by business from coming for the last week, and has not used a catheter. I passed No. 10, which was a little tight.

5th. Urine quite clear; feels better than he has done for the last three years; nocturnal emissions much diminished.

13th. Has procured a No. 10 metal bougie, which he is to learn to pass for himself, and made his first attempt to-day. Nocturnal emissions have quite ceased, though he has occasional erections. I did not see the patient again, owing to his dread of having an instrument passed, until the middle of March, when he sent for me, and I found that he had contracted a gonorrhœa, and had orchitis. For this he had been treated by his brother, and the acute symptoms had gone off. I therefore strapped the testicle, and he was soon able to go into the country. Says that he passes a capital stream of urine without any difficulty, but will not let me pass an instrument. Up to the present time he has not required further treatment for the stricture.

For the opportunity of treating the following cases I am indebted to Henry Bullen, Esq., the surgeon to the Lambeth Infirmary. The patients having been more or less under this gentleman's observation up to the present time, it is satisfactory to find that their condition is exceedingly good, and that they have required no further treatment.

Case 2.—Thomas J., aged 57, was admitted into the Infirmary of the Lambeth Workhouse, in Nov. 1859, under the care of Mr.

Bullen. His complaint was stricture and perineal abscess, which arose from an accident at the London Gas Works nearly six years ago, when he fell astride an iron pipe. Originally under the care of Mr. Cock, of Guy's Hospital, he was discharged after the successful use of the catheter about twenty times, relieved, but not cured.

At the time of his admission into the Infirmary there were several sinuses opening in the perineum, and one above the pubes, through all of which urine at times passed. The stream which came by the natural passage was exceedingly small and thread-like. With rest and medical treatment these sinuses have all healed, except one in the perineum, through which from time to time urine will find its way. The cicatrix is hard and knotty, and communicates to the fingers a sensation of cartilaginous density.

After some ineffectual attempts to introduce catheters of small size, he was by my advice placed under the influence of chloroform on the 23rd of February, 1860; and having passed a grooved director though the sinus into the bladder, I proceeded to force a No. 5 staff through the stricture and tough cicatrix as far as possible in the course of the urethra until it impinged upon the director, which served to guide it into the bladder. This was accomplished with considerable difficulty, and subsequently a No. 3 silver catheter was substituted for the staff, and was tied in.

During the succeeding night, owing to the patient's restlessness, the catheter slipped out. The dilatation of the stricture, which had been so far effected, proved, however, of the greatest service to him, as he was able to pass in consequence a pretty full stream of urine. Several ineffectual attempts were made to re-introduce an instrument, and therefore on the 17th of March I had him placed under chloroform, when I was enabled to introduce Mr. Holt's dilator with some difficulty. The stricture was successfully split up, a No. 10 catheter readily introduced, and the bladder evacuated, after which the instrument was withdrawn. A full dose of opium with hot brandy-and-water was administered immediately after the operation, under the influence of which he passed a good night.

March 18. A No. 9 catheter was used with comparative ease, it being found impracticable to introduce a larger size without force. In the afternoon he had a rigor, which was treated with the warm bath and hot brandy-and-water.

19th. Patient progressing favourably, and passes a good stream; catheter not passed, owing to patient's being asleep.

21st. No. 10 catheter was passed, and its use was continued occasionally for some weeks. The sinus in the perineum entirely closed, the thickening along the urethra totally disappeared, and the patient was able to resume his work.

Case 3.—John W., aged 61, admitted into the Lambeth Workhouse Infirmary, under the care of Mr. Bullen, on the 8th of December, 1859, suffering from stricture caused by falling off a ladder astride a

piece of timber at Exeter, about twenty-four years ago. He was then under the care of Mr. James, of Exeter, three weeks, and was apparently quite well until 1842, when he was admitted into Guy's Hospital with stricture, and was under the care of Messrs. Cooper, Key, Cock, and Callaway, who were unable to introduce a catheter into the bladder. In 1847 he was admitted into University College Hospital, under the care of Mr. Quain and Mr. Liston, and a No. 5 catheter was introduced by the latter surgeon on one occasion only, every subsequent attempt being unsuccessful. In the early part of 1859 he was admitted into the hospital at Melbourne, Australia, under the care of Mr. Ellis, and was discharged as incurable.

After several ineffectual attempts to pass a small catheter, he was placed under chloroform, and I succeeded in passing No. 1 silver catheter, which was tied in; and on following days Nos. 2 and 3, which were also tied in. Subsequently the instrument was removed for a few days, and the urine passed in a small stream.

On the 17th of March, 1860, the patient was again put under chloroform, and Mr. Holt's dilator having been introduced, the stricture was split up and a No. 12 catheter introduced. This was followed up by the use of an instrument on alternate days for a few weeks, when the man was discharged cured, and remains so, having been under observation up to the present time, being actively employed in the workhouse at his trade as a carpenter.

Case 4.—Henry L., aged 60, admitted into the Infirmary at Lambeth Workhouse, under Mr. Bullen's care, on the 10th of April, 1860, suffering from stricture and perineal abscess, the supposed cause being repeated gonorrhœas twenty-five years ago. He was admitted into St. Thomas's Hospital, and was under the care of Mr. South seven weeks, six or seven years ago. Was admitted into the same hospital a second time in November, 1858, and was under the care of Mr. Macmurdo four months; but a catheter was never introduced into the bladder.

May 3rd, 1860. He was placed under chloroform, and I introduced No. 4 silver catheter, after which Mr. Holt's dilator was employed, and the stricture having been split up, No. 10 catheter was readily introduced. The use of the catheter was continued for some time, when he was discharged cured, with the fistula healed, and up to the present time he has remained quite well.—*Lancet, August* 31, 1861, *p.* 203.

65.—ON INCISION OF STRICTURE OF THE URETHRA.

By Dr. JAMES ARNOTT.

Notwithstanding the great attention and research which so common and so serious a disease as stricture of the urethra has naturally excited, it must be acknowledged that our means of removing it are, as yet, very far from being perfect. Those which are safe are often

inefficient, and those which are efficient are so unsafe as to render their use justifiable only when the patient is exposed to more hazard from the disease than from the remedy. An investigation, however, into the sources of this danger, has shown that it is not an inevitable concomitant of the essential part of the proceeding referred to; and the purpose of the present communication is to explain how, by employing in its performance an instrument constructed on an entirely new principle, danger may be altogether avoided.

Although a catheter or bougie will, in the majority of strictures, give great relief, this is rarely of long duration; and many cases occur of an unyielding, irritable, or elastic nature, in which these means of dilitation are of little avail. A persistence in their use may, indeed, keep the urinary canal open to a certain degree, but as they do not subdue the irritation caused by the presence of stricture, the long continuance and extension of this irritation too often undermine the general health, and produce organic changes in the bladder, kidney, and other contiguous organs, which embitter and shorten life.

Incision of the stricture by a sheathed knife passed along the urethra, was, in former times, a practice occasionally resorted to. In consequence, however, of the rude and hazardous modes of effecting it, this kind of treatment fell into disuse, and at the time of the publication of my Treatise on Stricture, it was almost forgotten. What was there said and suggested on the subject, again brought it into notice, especially in France, and though the instruments employed for practising internal incision have been hitherto constructed and used on a bad principle, much good, on the whole, has resulted from the revival of this method of treatment.

The imperfections of these methods of internal incision led to Mr. Syme's improved external incision, which was proposed as a substitute for them. But the hopes once entertained that permanent relief from the effects of severe stricture could thus be obtained with safety, have not been fulfilled. As has happened with respect to some other medical questions, the statistics published in favour of the perineal section have not been properly or fairly adduced. Although when employed in cases of little severity, and where dilatation would have sufficed, this operation has not often had an unfortunate termination, it has proved fatal in a large proportion of that severer class of cases in which alone its adoption is warrantable.

In France the perineal section has not been received as a substitute for internal incision; and so highly is the latter proceeding esteemed, that certain French writers speak of the palliative treatment of strictures by dilatation and its radical or curative treatment by this kind of incision, just as they discriminate between the mere tapping of a hydrocele and its effectual treatment by irritating injections. But although relief from successful incision may last longer than that afforded by dilatation, this difference of duration has not been the

reason generally assigned for preferring it. It is only where, from the character of the stricture, relief could not be obtained by dilatation, that the majority of surgeons have deemed themselves justified in having recourse to a measure which, though it has proved less hazardous than the perineal section, has by no means been without hazard. Internal incision has often proved injurious or fatal by hemorrhage, infiltration of urine, and pyæmia. We are not indebted to its more zealous eulogists for our knowledge of these facts, but to the communications of impartial inquirers. M. Nelaton, for instance, whose position in Paris enables him to form a correct judgment on the matter, while admitting the necessity of having recourse to this proceeding in certain cases, states, in his recently published 'System of Surgery,' that "in more than half the number" of incisions there is hemorrhage, and often to an alarming extent. As this could not happen without the urethra being deeply or extensively cut, the other dangerous results can be easily accounted for.

If we reflect on the circumstances attending the most approved plan hitherto in use of cutting strictures from the interior, we cannot be surprised at these occurrences. The sharp blade having been protruded from (and at a considerable angle with) its sheath, after it has passed beyond the stricture, "is" (to quote some recently published directions) "firmly pressed on the floor of the urethra, and slowly and steadily drawn outwards about an inch or an inch and a-half." In this operation, the cessation of resistance to the progress of the knife furnishes the surgeon with the only reliable means of judging whether the stricture has been divided or not; for the elasticity of the urethra, or the ease with which it may be stretched, renders any previous measurement of very little avail. Now, if the knife is not opened exactly at the back of the stricture, and shut exactly in its front, the sound portion of the urethra will probably be more or less cut by it, as it is almost impossible, in the curved portion of the canal, and at so great a depth, to keep the protruded blade precisely in its axis; and so long as the resistance to the extraction of the knife continues, the operator will suppose that the stricture is being cut. When again, in the course of its retraction, the blade has reached the posterior part of a hard stricture, it will drag this to a certain extent forwards before penetrating it, and, in so-doing, will cause a gathering into folds of the portion of the urethra immediately in front of the stricture, through which folds the knife will be apt to start on the completion of its way through the hardened textures, however straight its course may be.

If this exposition of the cause of the hemorrhage, &c., be correct; if the mischief proceed from the sound portion of the canal being cut, as well as the non-vascular, consolidated and contracted part, it becomes a very important question, whether some method cannot be devised of cutting the latter part exclusively. If the division of the contracting tissues could be exactly limited, the objections to internal

incision would be removed, and a great desideratum in surgery supplied.

I have ascertained that this can be effected, either by employing a knife rendered so blunt as that, when employed in the usual way, it will only penetrate the part which is much pressed by it; or by employing a sharp knife in such a manner as that it will cut by pressure alone, or without any forward or backward motion. Both may be said to carry out the principle of incision being effected only on the application of great pressure.

In an essay on stricture, printed five years ago, I suggested an instrument of the first description; four years afterwards, Mr. Marshall proposed an urethrotome, closely resembling this instrument; and in March or April of the present year, a paper was read before the Academy of Medicine in Paris, by M. Maisonneuve, suggesting, for the third time, the application of the principle on which those were constructed.

In the 'Medical Times and Gazette' for February 2 last, I described, and illustrated by an engraving, another instrument for this purpose, to which, as being very superior to the first, I shall, on the present occasion, confine my attention. It consists of a flat tube, about seven inches in length, to one end of which a narrow and deeply-grooved director is attached, adapted for entering the stricture. Three long steel rods or wires, of a size that will pass through the flat tube, and one of which has, for an inch at the end, been sharpened into the form of a knife, constitute the rest of the apparatus. When it is used, the sharpened rod is passed along the tube into the grooved director, from which it is then pressed transversely against the stricture by pushing one or both of the other rods or wires between it and the whole length of the side of the tube. A very sharp knife thus pressed directly against the stricture, if the breadth of the tube allows it to move through sufficient space, will cut by the pressure alone, or without any retractile motion being superadded; but if the space be too small for this amount of pressure, a blade not so keen is preferable, and then a slight backward or sawing motion is required, in addition to the pressure, for the incision of the stricture.

As an explanation has been given, in a foregoing paragraph, of the accidents which happen from the use of the common urethrotome, there will be no difficulty in comprehending why the new instrument must be perfectly safe. The knife in the act of cutting is parallel, instead of forming an angle, with the axis of the urethra; it moves in a small definite space, which may be precisely that occupied by the previously measured stricture; no dragging of the stricture is caused by its motion, or gathering of the urethra into folds, because both its direct or transverse pressure on the stricture, and the end of the flat tube, keep it in its usual position, while the incision is being made; this motion is so limited, that even were it to displace the stricture, it would hardly be beyond the tenth of an inch; and, lastly, when

the knife is pressed out of the groove, no injury can be received by the sound or uncontracted part of the urethra, because, by giving no resistance. it is only pushed aside by its edge. In fact, the incision is as exactly limited to the stricture as it would be if the urethra were laid open and exposed to the surgeon's view. Unless the incision required should extend beyond its normal dimension, it is impossible to injure the sound portion of the canal, but the cases must be very few, if any, in which so extensive an incision is advisable. The sound portion may come in contact with the edge of the knife during incisions of strictures within this limit, but as there is no pressure upon it, there is no cutting.

The first case in which this urethrotome was used had been previously adverted to in page 18 of the Essay above-mentioned. It is there stated that the patient had laboured under stricture for many years, and that frequent dilatation was required to preserve a tolerably free channel for the urine, and thus prevent irritation of the bladder. This dilatation could never be carried beyond a very limited extent, and as there was constant impending danger from persisting irritation, the application of some auxiliary means had been frequently and strongly recommended. Relief, however, being always obtained for a time by the dilatation of fluid pressure, and as I could not *then* assure the patient that incision was without hazard, he preferred the safer, though less effectual measure. At last an urinary abscess formed in the perineum. and the urine spread from it to the scrotum. When I saw the patient, on his arrival from the country, he was prostrated by his sufferings. It was, notwithstanding, necessary to get rid of the urine and internal sloughs by extensive incisions; but as these were made under congelation in lieu of chloroform, the existing debility was not thereby sensibly increased. When the system had recovered itself from this perilous accident, the stricture was cut by the new urethrotome on two several occasions. with a few day's interval; and I was able afterwards to pass a flexible metallic bougie nearly twice as large as any that had been formerly used. Owing to the hard and callous nature of the stricture, the patient scarcely felt the cutting, and the loss of blood did not, on either occasion, exceed a few drops. When I examined the urethra, nine months after the incision, there was no recontraction, nor had there been any return of distressing irritation. The patient continued to pass, from time to time, the same sized instrument which I introduced after the last incision.

I will now add some remarks on the construction and mode of applying the instrument, which a considerable experience in its use has suggested.

Although I have not had occasion to use so small an instrument, a steel director, appropriate to a very narrow stricture, could, no doubt, be made as small as the grooved staff commonly used in the perineal section. The knife fitting its groove must necessarily be very small,

but if the knife be short, and if the supporting rod first passed under it be narrow, it will be sufficiently strong. There are few strictures, however, which cannot be previously dilated to the extent necessary to admit a larger silver director.

The flat tube to which the director is attached may have various breadths according to the degree of pressure desired. That which I have generally used is one-third of an inch broad. It must be remembered, however, that generally speaking it is only so much of the circumference of a stricture as is much pressed upon that will be divided by the knife; beyond this extent the pressure will be insufficient, and the stricture will yield to the knife instead of being cut by it. A knife separating from the director to the extent of two or three-tenths of an inch, even admitting that the channel of a narrow stricture is completely filled by the director, will not, perhaps, penetrate to more than half this depth. It may be proper, therefore, to make several incisions in different directions. If the same instrument be employed for these, the bulk of its director must be increased in proportion as the canal is opened, by a coating of sealing-wax applied after the metal has been thoroughly cleaned and heated.

In many strictures, however, one incision will be sufficient; they will yield afterwards to dilatation. One would expect this to be especially the case in those consisting of an elastic band.

It is questionable whether, as respects permanence of relief, there is advantage from extending the incision through or beyond the whole substance of a stricture. In the perineal section the incision extends through the whole of the subjacent parts, and must necessarily have always one direction, whatever may be the situation of the opening through the stricture. The channel for the urine preserved afterwards by the catheter, must be formed as much by the soft and yielding adjoining parts as by the urethra itself; and the hardened tissues originally forming the stricture will bear, therefore, but a small part of the pressure of dilating instruments. It arises probably from the increase of the morbid tissues being thus unrestrained that strictures have sometimes appeared to be eventually more untractable after the perineal section than they were before.

When the tissues forming the stricture have, by length of its duration or other causes, become of a cartilaginous hardness, repeated strokes of the knife may be required for its division. This can be easily effected by the new instrument, and by it alone.

Although tissues when so consolidated can hardly be infiltrated by urine (and it is in such cases that incision is especially appropriate) I have, nevertheless, as a precaution against the possibility of such an occurrence, generally recommended that a catheter should be occasionally used during the two succeeding days. A large bougie, a fluid dilator, or the dilator constituted by a modification of the new instrument itself, should be daily passed for some time afterwards, to prevent the adhesion of the cut surfaces.

The ordinary mode of using this apparatus is as follows:—It is satisfactory, though not absolutely necessary, to have made a previous examination of the stricture, and to have ascertained its length and the situation of its channel as respects the axis of the urethra; its distance from the orifice should be marked on the flat tube. Then, having warmed and oiled the instrument and inserted the blunt rods in the tube in such a manner that one of them, having its end of a wedge-form, may, when protruding from the tube, afford a sloping surface, it is introduced into the urethra. As soon as the grooved director has been lodged in the stricture (which may be ascertained, amongst other ways, by noticing when one or both rods are forced backwards), the outer one is withdrawn, and again pushed into the tube on the other side of the remaining rod so as to press this outwards. If there is a difficulty in doing this; if the patient feels the rods distending the stricture; or if the instrument be firmly fixed in the urethra after the second rod has been inserted, there will be little doubt that the director has been properly placed, and that it is of a proper bulk. If none of these circumstances are present, it must be inferred either that the director is not within the stricture, or that it fills its channel so imperfectly, as to render it probable that the knife when introduced will not be pressed with sufficient force against the stricture to cut it. When the surgeon is satisfied that the instrument is properly placed, both of the blunt rods are to be withdrawn, and the sharpened one is to be passed along the groove as far as is deemed necessary. Then, while the instrument is held so that its axis may correspond with that of the urethra, the knife is pressed or driven against the stricture, by passing one or more blunt rods or wires between it and the side of the tube. If this pressure be alone insufficient to effect incision, it is necessary to give the knife a slight retractile or outward motion, after which it is pushed again into the groove and extracted. The ease with which the knife has moved, its being soiled with blood, and the facility with which the blunt rods can again be introduced, are all evidences that the stricture has been cut. If these be wanting, the presumption will be that the stricture is peculiarly hard or very elastic, and that it will be necessary, in order to divide it, either that the knife should be repeatedly drawn over it, or be applied with greater pressure by means of a broader tube.

I trust it will not be supposed, from my having thought it advisable to give such minute directions, that this proceeding is one of difficult execution. But, as the most simple surgical operation may be so imperfectly performed as to prevent the attainment of its object, I am anxious to prevent such a result by mentioning everything of importance relating to an instrument which has little in common with other urethrotomes. Besides, several of the above observations regarding incision by this apparatus have an equal bearing on the employment of those modifications of it which furnish us with an excellent dilator

of ex-centric action, and a certain mode of applying caustic to the front or the channel of a stricture. For it must not be forgotten that, however valuable a perfectly safe mode of cutting stricture may be in those cases which resist dilatation, it ought not to be had recourse to until this has been properly tried and found inadequate; and it ought also to be borne in mind that, when incision has been employed, it is necessary to maintain the benefit resulting from it by the occasional use, for some time at least, of the most efficient means of dilatation.—*Med. Times and Gazette, Sept.* 14, 1861, *p.* 264.

66.—ON PHYMOSIS.

By P. C. Price, Esq., Surgeon to the Great Northern Hospital, &c.

[The author performs the operation for phymosis somewhat differently from most surgeons.]

In young children he usually advised the exhibition of chloroform, as much pain was not only thereby avoided, but it was exceedingly difficult, otherwise, neatly to finish the operation, owing to the contortions into which the little patient generally twisted his body. The prepuce being put on the stretch, so as to remove the orifice as far from the glans penis as possible, the blunt-pointed blade of a pair of scissors is passed through the preputial orifice to the distance of about a quarter or half an inch towards the corona glandis, and the prepuce divided through its upper surface. This will allow the glans to be seen, covered, in all probability, especially if the phimosis be congenital, by the mucous membrane of the foreskin, which will be found more or less tightly adherent to the glans. Laying aside the scissors, this adhesion is to be separated by somewhat forcibly retracting the back portion of the prepuce, and by means of the thumb-nail breaking through such points of union, as may be more than usually firm. Around the fossa in front of the corona glandis will generally be found small patches, or masses, of hardened and otherwise altered secretion of the odoriferous glands. These being removed, the scissors must be again employed, and such portions of the redundant skin and mucous membrane cut away as will allow of the glans penis, after the edges of the wound are united, remaining uncovered. In removing the necessary portions of integument, the author generally contrives, by shaping the incision somewhat obliquely from above downwards, to avoid wounding the artery of the frænum, whereby there is little risk of secondary hemorrhage and infiltration of the loose tissue with blood —a result which he has frequently known prove troublesome, and for a considerable time retard the healing process. If, however, it be desirable to loosen the connexion of the mucous membrane with the glans at that part which is called the frænum, instead of dividing the little artery which runs in this direction, it is better to separate, with the scissors, the union of the frænum with the glans immediately in

the line of junction, by which means the course of the vessel will be avoided. Mr. Price, although some surgeons do not follow the practice, is in the habit of keeping the mucous and cutaneous surfaces in contact by sutures, unless the patient on whom the operation is performed be very young. Sutures of the very finest silk are to be preferred to those in ordinary use, and made either of the same material or of metal, as they cause less irritation, and do not need removal before their destruction has been effected, in the course of a few days, by reason of the contact of the healing or other discharge. To prevent the urine, for the first few hours after the operation, from excoriating the edges of the wound, it is advisable to smear them with oil or cerate. In performing such an operation, less danger will be run of wounding the glans penis—a result which the author has known to occur when the bistoury has been used. It must be recollected, that to obtain complete relief from a congenital phimosis, a mere slitting up of the elongated prepuce will not generally suffice; but a certain portion of the mucous membrane and cutaneous tissues must be removed.—*Lancet, May* 4, 1861, *p.* 436.

67.—LITHOTOMY—LATERAL OPERATION.

Case under Prof. FERGUSSON, at King's College Hospital.

The patient, a boy about five years of age, was placed under the influence of chloroform. Mr. Fergusson, after having performed this operation in his usual manner by lateral section, observed that the stone had no phosphates about it—was of slow growth, about six months'. Mr. Fergusson on this occasion again insisted on the necessity in young subjects of cutting well into the membranous part of the urethra. If this precaution was not observed, he said, the prostate was liable to be pushed on end of finger into the bladder, thus inverting the order of things. Thus, not only the anterior, but posterior wall of urethra might be divided, and its continuity destroyed. Mr. Fergusson said, operators often, when they have reached the groove of staff in membranous part of urethra, as urine escapes, think they have reached the bladder. "No such thing: the membranous part of urethra, somewhat like a syphon, is generally, if not always, filled with urine—the groove of staff acting as a conductor of the urine. If you do not cut fully onwards into the prostate, so that you can easily pass your finger into bladder, a complication will assuredly happen. I consider the lateral operation as good as any other: other modes are introduced—I shall abide by the lateral cut. For simplicity of procedure, safety in execution, freedom of action, and ample space of opening—these circumstances all recommend the lateral operation to me before all the other modes, especially in children. I use scoop in children in preference to forceps; but small forceps may be used on some occasions. We hear of quick recoveries; we are told of patients

walking about in the wards on the day following operation, or even in a few hours after. I caution you about these quick recoveries. *Above all things, avoid quick recovery after lithotomy.* Give your patient rest, quiet, and *decubitation*, for a week, fortnight, three weeks, or even a month, rather than attempt quick recovery."—*Medical Circular, July* 24, 1861, *p.* 64.

68.—RADICAL CURE OF VARICOCELE.

Case under the care of Henry Lee, Esq., King's College Hospital.

A patient, labouring under a very aggravated form of varicocele, was brought into the theatre on the 9th of Feb. last. It appeared that he had suffered for a considerable time from pain in the part, and in the loins. The testicle upon the affected side was evidently wasted; but the most remarkable feature in the case was the great distance that it hung below the other on the opposite side. The testicle on this (the left) side was so pendulous and loose, that when the patient lay on the operating table it hung over towards the outside of his left thigh. Mr. Henry Lee proceeded to operate in a way somewhat different from that in which the operation has been usually conducted. Having introduced a thin needle between the vas deferens and the enlarged veins, a figure-of-8 ligature was passed over the ends of the needle, and the bunch of veins was thus compressed. The same proceeding was repeated at the lower part of the scrotum. In each case a piece of thin leather was placed under the thread to prevent any pain that the pressure on the skin might occasion. A considerable extent of the skin of the scrotum and of the enlarged veins beneath it were thus included between the two needles, and the circulation in these parts in a great measure obstructed. A thin knife was then introduced about midway between the two needles, and between the vas deferens and the enlarged veins. The knife was directed upwards, so as to divide the integument near the upper needle. A second incision was made to pass downward, so as to divide the skin near the lower needle. A wedge-shaped portion of tissue—skin, enlarged veins, and all—was thus removed, and a very considerable interval was left. Some extremities of divided veins, and some cellular tissue, were removed with scissors, and a clean surface left. It was remarked that although such an extensive surface was exposed in so vascular a part, yet no hemorrhage followed; this, in fact, was completely controlled by the acupressure of the needles. The needles were now drawn together and maintained in position, and thus approximated the edges of the wound, which was then dressed by a piece of dry lint, and the patient sent to bed.

Mr. Henry Lee remarked that cases of pendulous testicle, as they present themselves in practice, may be divided into three classes: 1, those in which the skin and subjacent tissues are relaxed; 2, those in which the veins alone are enlarged; and 3, those in which both

skin and veins are relaxed and enlarged. The first may be remedied by the removal of a portion of the integuments only; the process of cicatrization braces up the surrounding parts, and the evil is thus remedied. In the second class of cases the obliteration of the veins is requisite, and this may be done by subcutaneous section, without any wound in the skin beyond that which is required to admit the thinnest knife. But in the third class, where the skin is much relaxed, the simple obliteration of the veins will not remedy the pendulous condition of the testes. In such instances Mr. Lee recommended an operation similar to that which he had now performed—namely, the removal of a portion of the skin, together with the subjacent enlarged veins. The acupressure of the needles he considered sufficient to ensure the patient against hemorrhage on the one hand, and any absorption through the veins on the other. The operation, therefore, although apparently a formidable one, was, he believed, free from danger.

The patient in the above case, we understand, left the hospital at the end of three weeks, and continued to present himself occasionally as an out-patient. The wound cicatrized very firmly, the enlarged veins were completely closed, and the testicle retained very much in its natural position; the testis also, which had been wasted before the operation, regained its normal size.—*Lancet, June* 22, 1861, *p.* 610.

69.—*On the Radical Cure of Varicocele.* By J. Redfern Davies, Esq., Surgeon to the Birmingham Workhouse.—[The following short case is one of some interest:]

The patient, aged seventeen years, was admitted into the infirmary on the 10th of December, 1858. He was the subject of varicocele on the left side, which had existed for the last three years, and to so great an extent as to prevent him from following his employment. Being desirous of radically curing him, I adopted the method of M. Ricord, which I had seen him perform with success; but instead of the hempen ligature which he used, I tried to do the same thing with silver wire—namely, between the vas diferens and the veins I passed, by means of a needle, a double silver wire, and by the same apertures in the opposite direction, behind all the veins, another wire. By engaging one free end of the wire through the loop of the other one, and doing the same on both sides, the veins were, by traction on the wire, compressed. This traction was kept up constantly by means of an elastic watch-spring, bent in the shape of a horse-shoe.

In ten days' time, thinking the varicocele cured, and wishing to remove the wires, I tried to do so, and found I could not, though it was easy enough with hempen ligatures. I therefore cut off the wires as short as I could, trusting to the innocuity of silver wires to cause no harm. In another week the punctures were well healed, and he was discharged, the varicocele then appearing perfectly cured.

Seeing him again in about a year's time, upon examination I found that all things remained as when he left; and he stated he had resumed his work immediately upon leaving. He himself was totally unaware of there being any wires in his scrotum.—*Lancet*, *July* 20, 1861, *p.* 60.

70.—NEW REMEDY FOR IRRITABLE CONDITIONS OF THE BLADDER.

By Henry Thompson, Esq., Assistant-surgeon to University College Hospital.

[Mr. Thompson has been trying the root of a grass called the *Triticum repens*, in irritable states of the bladder, with success. He says:]

My first acquaintance with it was derived about three or four years ago from a gentleman in the country, who was the subject of severe and long-standing stricture of the urethra, and who had long used it to relieve the frequent and painful micturition of which he was the subject. In this case no drug in the Pharmacopœia afforded so much relief as the remedy in question; but this circumstance I was at first inclined to attribute rather to imagination, or to a peculiar idiosyncrasy, than to the virtues of the plant. However, on his strong and repeated representations respecting its value, not only to himself but to some others whom he had supplied with it, I was induced, not until about a year ago, to prescribe it in a few cases, and subsequently gave it a systematic trial on a large scale, both in hospital and in private practice.

The form I have adopted has been uniformly the same. One ounce of the dried and cut stem is infused in a pint of boiling water for an hour. The liquor removed by straining has been given, unmixed with any other remedy, in quantities varying from twelve ounces to a pint during the 24 hours, in several doses. The taste of the infusion is rather agreeable than otherwise; it produces no nausea or derangement of the stomach.

From notes of the numerous cases in which I have employed it, making at the same time due allowance for the effect of other sources of benefit to the patient, I have arrived at the following conclusions respecting the indications for its use:—

In vesical irritability produced by inflammation of the prostate and neck of the bladder; in severe gonorrhœa, and especially when the inflammation extends backwards; in the pain and spasm caused by calculus, and by aggravated stricture of the urethra, as well as in some cases of obscure disease of the bladder, the good effects of the infusion have been very marked, and it has proved far more efficacious than the buchu, which may fairly be esteemed the most widely applicable and generally useful of our officinal remedies of this class in such circumstances. In cases of prostatic enlargement in elderly patients

it has been of service, but less frequently than in the conditions above-named. It has also afforded great relief in renal calculus. A medical man practising in London, who has thus suffered, and very severely during many years, tells me that "after trying every approved remedy, it is the only thing that has rendered life endurable." This is one of four similar cases in which it has been more or less useful.

In short, wherever micturition is very frequent or painful, depending on hyper-sensibility of any part of the urinary passages from acute or subacute inflammation, with signs of its presence in the urine itself, the symptoms are mostly materially relieved, and the urine becomes clearer. If improvement is produced at all, it is generally very soon after commencing the medicine, and if none can be observed in four or five days, it is not worth while continuing to employ it.

I believe it is important that the plant should be gathered in the spring, shortly before the leaves appear; the stem is then to be slowly dried without artificial heat, and cut into short lengths for use. The infusion obtained from material so treated is superior to that made from plants gathered indiscriminately at any time, and also to the infusion made from the Triticum repens which is imported by the herbalists for the purposes of French pharmacy in this country.—*Lancet, Oct.* 12, 1861, *p.* 345.

DISEASES OF THE SKIN.

71.—ON TUBERCULAR DISEASE OF THE EXTERNAL LYMPHATIC GLANDS.

By P. C. Price, Esq., Surgeon to the Great Northern Hospital, &c.

It not unfrequently happens, when ulceration follows suppuration of tubercular glands, and heals without the direct attention of the surgeon, that the implicated integument remains disfigured by induration, unseemly scars, and uneven cicatrices. Under such circumstances, the use of certain measures, to be presently described, will oftentimes lead to a favourable solution of the existing disfigurement.

Lotions. When induration of the integument and adjacent cellular tissue results from the healing of tubercular ulcerations, it may frequently be removed by absorption, especially when it is of recent origin, and correct therapeutical means are employed. Without doubt, solutions of various salts of iodine will be found among the most valuable adjuncts which the surgeon has at his command. Mr. Erichsen, in his 'Science and Art of Surgery' (3rd edit., p. 465), strongly recommends lotions of iodide of potassium and carbonate of potass: a drachm of each of the salts, with an ounce of spirits of wine and eleven ounces of water, makes, he says, an excellent application, which should be kept constantly applied to the implicated parts by means of linen or lint covered with oil-silk.

I am in the habit of substituting iodide of ammonium for iodide of potassium, as I have reason to believe that it will be found more beneficial. Sir Astley Cooper placed great faith in a solution of chloride of ammonium; and other surgeons of large experience have written in favour of the use of simple alkaline solutions. Whatever liquid applications are employed, it is all important that sufficient time should be given for a trial of their efficacy; for it oftentimes happens that a satisfactory result is only obtained after their most persistent use.

Unguents. When the application of lotions has failed to produce the desired effect, it will be advisable to have recourse to some more definite measures to procure absorption and removal of the indurated condition. I have experienced considerable advantage from simple frictions, especially when no abnormal tenderness has been exhibited by the implicated parts. But still greater good will oftentimes be derived from the additional use of certain ointments and oils.

When the aplastic deposits are of long standing, the use of an ointment composed of iodide of ammonium, such as already described, will often lead to gradual softening and absorption of the hardened surface to which it is applied. The great advantage of this application is, that it can be used with freedom without imparting to the skin the peculiar colour which the local employment of pure iodine produces, and whilst equally effective, or nearly so, is, in many ways, less objectionable to the patient.

When it is deemed necessary to make use of mercurial preparations, the *unguentum hydrargyri* may be selected; or, under certain circumstances, iodine and mercury may be combined in one or other of the forms previously considered. I have elsewhere spoken favourably of the use of oils, when assiduously rubbed into the integument covering an indurated gland; and I can also recommend their employment in all false hypertrophies of the skin and cellular tissue. When containing iodine, mercury, or certain stimulating substances, such as ammonia, camphor, chloroform, &c., they will be found doubly advantageous. Great care and discrimination are, however, always needed before advising friction either with ointments or oils. I have known too vigorous and oft repeated applications of these substances fraught with the greatest mischief; while it not uncommonly occurs that even a limited use of them produces an undue amount of irritation, which leads to ulceration and suppuration, and, in fact, to a reopening of the original sore. When such is the case. I have found occasional paintings with glycerine, in which is dissolved iodide of potassium or of ammonium, of great benefit.

Caustics. As already seen, an unseemly scar is one of the most frequent results which follow an indifferently treated attack of suppurative destruction of the external lymphatic glands. When a disfigurement of this nature causes not only personal but physical annoyance, the opinion of the surgeon will oftentimes be solicited as to the best

method of obtaining its removal. Scrofulous scars (so called) generally present certain characteristics. They vary in colour from a nominal to a bluish-purple tint, have a raised and unhealthy appearance, and are sparingly covered with cuticle. Under certain conditions, they prove irritable and painful, and are always more or less liable, especially when the system is impoverished, to assume diseased changes by reason of their low standard of organisation.

When a scar resulting from the healing of a tuberculous ulcer, or one in connexion with a gland which has been involved with tubercle, is of such a kind as to give rise to disfigurement, and becomes the seat of irritation and pain, its destruction may be deemed advisable, so that nature, immediately assisted by art, may more efficiently repair such structures as have through disease lost their original integrity. For this purpose, the use of various caustics has from time to time been recommended. Among those most generally selected, the following may be mentioned:—Chloride of zinc, potassa fusa, nitrate of silver, and the strong mineral acids. I am most disposed towards the employment of potassa fusa, and seldom select any other caustic, as I have found it, when judiciously employed, in every way the most satisfactory. One or two applications usually suffice; and, if care be taken, the new tissue which results will bear a very close resemblance to healthy skin, although to the accustomed eye it will always, or generally so, be found to differ. It usually possesses a more shining and glistening hue, and presents a stretched appearance. Wandering over its surface will be detected irregular, tortuous, red, linear vessels; and here and there, especially if the tendency to scrofulous action be prominently marked, a proneness to ulceration. Although the patient may be well pleased to recover from a severe attack of glandular tuberculosis with no further disfigurement than one or more patches of new tissue, which if not absolutely similar, bear a close resemblance to the normal cutaneous surface, still the surgeon is bound to use every endeavour to forward the most advantageous form of cicatrisation and reparation.

It is only of late years that a correct acquaintance with the nature and nutrition of scars and cicatrices has sufficiently impressed practitioners with the importance of regulating and avoiding their unnecessary formation. There is no doubt whatever that the growth of cicatrices is accomplished by assimilative processes identical with those which lead to the development and increase of healthy tissues. Consequently, it is expedient that all available means should be used to keep them as limited as possible; for although, in the early years of childhood, a scar resulting from tuberculous destruction of a lymphatic gland may be nearly imperceptible, still, at a more advanced period of life, it will be found to have relatively increased with the other tissues of the body; or, to use the words of Mr. Paget, "the scar of the child, when once completely formed, commonly grows as the body does, at the same rate and according to the same general rule; so that a scar which the

child might have said was as long as his own forefinger, will still be as long as his forefinger when he grows to be a man." ('Op. cit.,' vol. i. p. 49.)

Bearing these facts in mind, I make it a point never to resort to the use of caustics for the destruction of scars and cicatrices, unless I can be tolerably certain that improvement of an appreciable kind will result. In general, the pure caustic potassa, in stick, although immediate and sometimes severe in its effects, will be found the most advantageous form; for, with care, the diseased tissues can with nicety be destroyed without in any way endangering those of a more healthy nature. When the cicatrices are numerous, and situated, as they usually are, on the neck, it is generally more convenient to destroy them by degrees; for, in this way, the surgeon will be better able to accomplish what always proves to be a delicate, and indeed, sometimes no very satisfactory task. After the formation of an eschar, soothing applications will be found most grateful. In the course of a day or two, the destroyed portions will separate, and leave a healthy surface. If this surface be on a level with the surrounding skin, it will be easy to induce cicatrisation and a permanent improved condition. Should one application of the caustic prove insufficient, it may be repeated, but not without giving some slight pain. The practitioner will, however, probably encounter but little difficulty in the judicious destruction of such scars and cicatrices, which cause an amount of disfigurement unpleasant to the sufferer, and by no means creditable to surgery, provided due attention be paid to certain requirements which have been noticed in these pages.

The Knife. When the cicatrices which result from the healing of suppurative lymphatic glands are very extensive, and cause serious disfigurement, they may oftentimes be removed, or considerably lessened, by a judicious and skilful recourse to the knife. The peculiar puckered condition which these cicatrices assume has already been alluded to; but it remains to be noticed how position and various circumstances influence their formation. A cicatrix following destruction of one or more of the superficial cervical glands situated in the middle of the neck, assumes a permanent puckered and dimpled appearance, by reason of the skin being glued to the deeper structures; for it will frequently be found impossible to elevate the integument so implicated from the parts placed immediately beneath. When lymphatic glands situated near to bone are destroyed, and where there is a paucity of soft covering materials, the cicatrices resulting will be likewise found adherent, not only to the cellular sub-structure, but to the bone itself. It is most usual to observe this condition at the base and angle of the lower jaw, beneath the eye, on the forehead, and over the clavicle.

When cicatrices of this character disfigure these various parts, they may be treated by the knife in one or other of the following ways.

If the cicatrix be healthy, and its unsightliness be mainly dependent

on its puckered and dimpled appearance which results from its adhesion with deeper tissues, it will oftentimes suffice to sever the bond of adhesion, and prevent, during the healing of the wound which has been made, a re-union of the divided parts.

An operation of the nature is slight, free from any great amount of pain, and oftentimes productive of much benefit.

The details of the following case illustrate how such a proceeding may be accomplished. A girl, aged 15, who had suffered for many years from various forms of scrofulous disease, experienced considerable inconvenience from an unsightly cicatrix, situated on the upper part of the right cheek, over the junction of the malar with the superior maxillary bone. The principal deformity arose from its firm adhesion to the bone. The tissue forming the cicatrix was very thin, and there appeared to be little, if any, interposed structure between it and the bone, while the patient had a fat chubby face, so that the deep depression which thus resulted was rendered the more disfiguring. A thin-bladed narrow knife (a tenotomy knife) was inserted at the distance of about a quarter of an inch from the outer margin of the cicatrix, and passed obliquely down to the point of adhesion, which was then freely divided, care being taken to keep the point of the knife close to the bone. When division had been satisfactorily accomplished, the opposing raw surfaces were separated by drawing the cicatrix away from its old position, and retaining it in its new one by means of sticking plaster. In this way, the two surfaces were allowed to heal without the possibility of their again becoming united. In four days' time, the cicatrix was allowed to slip back into its former locality, when it was found that considerable improvement had resulted, although it was impossible, as it is in all similar cases, for the deformity to be quite removed, on account of the loss of cellular tissue, fat, &c., which had taken place.

I have resorted to a similar operation when the cicatrix has been situated beneath the margin of the lower jaw, and with even better results. The proceeding is, however, still more satisfactory when the band of adhesion is connected with soft parts only, as where the cicatrix is situated in the neck beside the sterno-mastoid muscle. When the cicatrices are more complicated, it is often expedient to resort to a somewhat more extensive use of the knife, and entirely remove the disfigured integuments. This may often be accomplished to the credit of the surgeon and satisfaction of the patient.

The operation consists in entirely removing the cicatrix, and uniting the lips of the wound in such a way that a mere linear scar results. The advantage of such an operation is at once evident, especially if the deformity to be removed be the only one situated in a part of the body usually exposed to observation.

I may quote the following case as illustrating the advantages to be derived from such a method of treatment. A young lady consulted me, in the autumn of 1859, on account of an ugly scar, about two

and a half inches in length, situated on the right shoulder, in such a position that she was unable to wear a low dress. Under the influence of chloroform, it was removed, great care being taken perfectly to adjust the margins of healthy integument, so that a mere linear seam should result. This was accomplished by means of delicate wire suture; admirable union ensued, and in a week, she left London, well satisfied with the result obtained.

I could quote many instances in which a judicious application of the knife was followed with decided advantages, but the above case is sufficient to prove the fact.

For the adjustment of the lips of the wound, I am in the habit of using a fine metallic pin, with the figure-of-8 suture, such as is employed by continental surgeons, in preference to the sewing needle armed with either silk or wire.

It occasionally happens that the cicatrix resulting from suppuration of lymphatic glands is so extensive that neither of the above operations will prove of any service. It then becomes advisable to remove the cicatrix and transplant a portion of adjacent healthy skin—in fact, a true Tagliacotian operation is needed. It is fortunate, however, that such a severe measure is seldom required; although I have resorted to it in instances in which the lower eyelid has been extensively disfigured, and serious conjunctivitis, and other affections of the eye, have resulted from inability to close the eyelids.

When mere bridles of integument cause additional disfigurement to a cicatrix resulting from destruction of a gland, they may be removed by means of the scissors or knife, without causing any special pain or annoyance. Situated on the anterior surface of the arm are one or more lymphatic glands, which sometimes become greatly enlarged and suppurate. The destructive action not unfrequently involves the flexor muscles and tendons, and the consequence is, that a serious amount of contraction of the fingers ensues.

I have very lately seen, in the practice of my friend Mr. Fergusson, a case illustrating these various features. The patient, a young lady, had suffered from scrofulous suppuration of one or more of the lymphatic glands in front of the arm. Two or three of the fingers were considerably flexed, and could not be extended, by reason of the contraction which had ensued in the muscle supplying the tendon—superficial flexor of the fingers. Division of the contracted tendons was resorted to, and the patient regained an excellent use of the fingers.

Such is a brief notice of the advantages which may often be afforded by the surgeon in cases of disfigurement and deformity arising from tuberculous and simple destruction of the lymphatic glands; but while I have endeavoured to point them out more forcibly than, perhaps, other authors on scrofulous diseases have done, yet I cannot too urgently insist on the importance of due attention being paid, both by the patient and the practitioner, to the various destructive and

reparative processes which ensue when the external lymphatic system is invaded with tubercle; believing that, in the treatment of this disease and its results, prevention is, on all occasions, better than cure.—*British Medical Journal, April* 6, 1861, *p.* 358.

72.—ON THE TREATMENT, WITHOUT EXCISION, OF WENS AND OF SOME OTHER CYSTS.

By M. A. Courty, Professor in the University of Montpellier.

There is no application of our art more frequent, and in some measure more routine, than the treatment of those tumours, often so numerous, and of dimensions so varied, which are developed on the head, in the substance of the scalp, or below it, in the neck, on the face, and some other parts of the body, consisting of a cyst and its contents, whatever may be the variable thickness and density of the envelope, the solidity or fluidity of the encysted matter.

Removal is a prompt and easy method of dealing with them; but in addition to the fact that it is not always exempt from accident, it is not readily consented to by patients. Not to mention the numerous cases in which the multiplicity of the wens makes the patients dread the renewal of the pain they have once unwillingly consented to undergo, these tumours being naturally painless, often little troublesome, not interfering with any functions, not threatening life, nor causing imminent danger nor real infirmity, it is natural that they should be borne, until their great development, inducing the inconvenience of a disagreeable deformity, or the danger of approaching degeneration, overcomes the timidity of the patients, and hastens their determination to submit to operation. It still remains for us to combat the prejudice entertained, that in touching a wen we may run great danger, not only of such local accidents, as erysipelas, inflammation, or epithelial development, which are real; but still further by the possibility of supposed metastases, or of some disturbance which their removal may excite throughout the system.

Setting aside, then, removal, which must sometimes be practised, it may be said that the destruction of the cyst is the key to every method, the application of which is capable of producing a permanent result. It remains to determine the best mode of destroying the cyst, and the cases to which the method is applicable.

I shall begin by observing that the destruction of the cyst does not necessarily involve that of the skin. It is well to make this remark, because the majority of surgeons who have applied agents of destruction, especially of cauterization, in the treatment of wens have applied them on the skin; differing not much in that from quacks, who, by means of so-called dissolving plasters, or of pretended chemical pencils, determine on the skin, throughout a variable extent, the painful formation of eschars more or less extensive, which may become, as

well as incision, the source of inflammatory symptoms. Whether, with M. Legrand, we employ linear cauterization with caustic potash, or, with Bonnet, superficial cauterization with Vienna paste, or pâte de Canquoin, or, with M. Maisonneuve, *cautérisation en flèche*, &c., the skin is always attacked. I have had occasion to see this cauterization followed by erysipelas; and even when the bottom of the cyst was thus exposed, without being destroyed like the surface, I have witnessed an epithelial vegetating production developed at the bottom, and soon assuming the aspect of an epithelioma, or of a cancroid, little encouraging to the operator, and necessitating a fresh and extensive application of the caustic.

I, therefore, lay it down as a principle that the skin ought to be, so far as possible, preserved, and that the destructive agent ought to be carried into the very cavity of the cyst.

In what mode are we to penetrate into the cyst?

Almost always in grubs and little wens, often even in voluminous tumours, we find a black spot indicating the seat of the neck of the orifice, which is supposed to be necessarily obliterated, and which is often only plugged. Nothing is easier than to remove the obstruction by pressure, or by the action of some very simple chemical or mechanical agent, a little soap and water, alkaline solution, the point of a probe, of a pin, or needle. We empty the cyst by pressing it; sometimes the thickened matter comes out gradually, in the form of a worm; sometimes, more fluid, it escapes in a jet, and is quickly and entirely expelled by the pressure of the fingers.

If this orifice is deficient, the point of a needle, or a little trocar, readily and painlessly substitutes for it an artificial opening.

Finally, if the cyst is very voluminous, if it is multiple, as I have seen in several instances, a double puncture, kept up by the presence of a seton during a few days, suffices to afford access to a single or multilocular cavity, and to give time and opportunity to express by degrees the entire contents, without exciting pain, or giving rise to any inflammatory symptom. To make this double puncture, and to introduce the seton, I prefer the trocar to the needle, the more rapid and less painful introduction of which forms openings larger, and less sensible to the contact of the thread. I then perforate the tumour through and through with a trocar, leaving the canula some moments *in situ*. Into this I pass the seton, which, therefore, does not touch the openings; then I withdraw the canula, and fix the seton by attaching its two extremities to each other. When the tumour is very voluminous (and I have been able thus to operate on some which were as large as a turkey's egg, or as an orange), I make use of M. Chassaignac's drainage trocar, the point of which, furnished with a notch to which the thread may be attached, allows the tumour to be perforated, the seton to be passed into the canula, and the latter to be withdrawn in less time than it takes to describe this little manœuvre.

The cyst having been emptied, how must we proceed to effect the obliteration of its cavity?

Strictly speaking, we may have recourse to one of two modes: either to modify its inner surface, to irritate it, inflame it, and determine by approximation and contact the adhesion of the opposite walls; or to produce its more or less rapid destruction, and its expulsion by the orifice, the cutaneous envelope quickly retracting, and gradually filling up, either by retraction or by deep adhesions, the void caused below it by the expulsion of the cyst. This latter mode is far preferable to the former.

At first, that is to say, about fifteen years ago, I was content to aim at simple modification of the walls of the cyst. To this end, after having expelled all the contents, I injected into the cavity various liquids. I had adopted, in preference to all others a strong solution of caustic potash (one part, and sometimes more, to fifty parts of water), thrown into the cavity by means of a small glass syringe. This solution had the advantage of first dissolving all the fatty or sebaceous matter which remained adherent to the walls, and then of irritating the latter, and disposing them to adhesion. Methodical pressure, continued or interrupted, favoured by the natural tendency to retraction of the skin and of the cyst, which were no longer distended by their contents, brought about by degrees the adhesion of the several portions of the sac, which eventually came into mutual contact, and the radical cure was finally attained after the lapse of a period varying from about fifteen to fifty days. On no occasion did pain or any other accident interfere with the treatment.

Since that time, thanks to the constant progress of cauterization applied to surgical operations, I gradually substituted for the caustic potash—of which I no longer made use, except to completely wash out the sac—agents more energetic, and more easily managed. Canquoin's paste, and particularly his various sparadraps, such as the school of Lyons has introduced into ordinary use, appeared to me perfectly to fulfil the object I proposed, and I have not ceased to employ them in the treatment of wens.

The following is the mode of proceeding:—A piece of sparadrap of chloride of zinc is cut of a length proportioned to the capacity of the cyst, narrow enough to pass through the opening; its penetration may be facilitated by rolling it into a cylinder, so that the caustic substance occupies the external surface of the long and slender cylinder. It is always easy to push it into the interior, the natural orifice of the wen admitting of its passage with the slight aid of a director, a probe, or the head of a pin; the artificial opening, if it exists, being so much the more convenient, as it must have, as we make it, more considerable dimensions. For grubs, a piece of Canquoin as small as a grain of millet, or a lentil, is sufficient; for the more voluminous wens, a cylinder from one to three centimetres in length will be required. If the cutaneous opening appears insufficient, the end of

the caustic is retained in it, instead of being forced entirely into the cavity of the cyst.

Pressure exercised on the wen at least twice a day gives exit, from this time, to a purulent matter, occasionally accompanied with small shreds of the cyst, and remains of the caustic sparadrap. But here two cases present themselves: either the cyst is small, very movable under the skin which covers it; or it is very large, and adherent to the tissue in a great number of points.

In the first case, that is to say, when it does not exceed the size of a large nut, which is not so very small, one cauterization is often sufficient to effect a cure. Whether this takes place after one or several introductions of the caustic, it is usually accompanied with the expulsion of the modified cyst. By the use of pressure, the latter makes its appearance at the opening, whence it is easy to complete its extraction with the forceps, when its expulsion does not occur spontaneously. We then obtain the cystic sac intact, the dead fibrous tissues presenting a whitish or gelatinous appearance. It is unnecessary to say that the complete obliteration of the cavity, the retraction of the skin, and its adhesion to the deep-seated parts, soon follow upon the expulsion of the sac.

If patients were well informed as to the facility of freeing them without pain or danger from these grubs and little atheromas, which threaten to become large wens, we might with certainty prevent the development of every tumour of this kind. To introduce through the natural opening of the grub, or through an artificial opening made with a needle, after having evacuated its contents, a small portion of caustic of the size of a lentil, and thus to destroy the cyst, the expulsion of which takes place almost spontaneously, would be a matter of a few days. We should thus always prevent a small, indolent tumour from ever becoming an inconvenient or painful deformity; and we should avoid exposing the patient to the sufferings and dangers of an operation, which at last becomes indispensable.

In the second case, that is to say, when the cyst is voluminous, of long standing, multilocular, &c., we cannot hope for a termination so rapid or so complete. We must then reckon on the successive expulsion of shreds of the cyst, which gradually mortify, or on the adhesion of the points of the sac which shoot out sufficiently, in consequence of the irritating action of the caustic, to unite one to another. The successive introduction, at some days' interval, of several pieces of Canquoin's sparadrap, methodical pressure exercised on the wen either continuously or interruptedly, care to keep it always emptied of the products secreted in it, and to favour the retraction of the skin, and the contact of the opposite surfaces, finally bring about by degrees a result as advantageous as that obtained in the first case at a less expenditure of time and money.

The duration of the treatment may then vary from thirty to fifty days, but the result is always cure; and the patients attain this end

without having incurred the least danger, or experienced the slightest pain. Contrary to what is true of the skin, the inner surface of the cyst has always appeared to me to be insensible to the action of caustics, all the benefit of which is consequently obtained, without our having to fear the reactions which follow the development of the pain inevitably connected with their application to the cuticle.

I have never observed, in the very great number of wens I have in this mode operated on, the production of a single one of the accidents which may follow any surgical operation.—*Bull. Gen. de Thérap., and Dublin Quarterly Journal, May* 1861, *p.* 487.

73.—ON THE PREVENTION OF PITTING IN SMALL-POX, BY THE APPLICATION OF LINIMENTUM AQUÆ CALCIS AND COTTON WOOL.

By Dr. Joseph Bell, Physician and Clinical Lecturer to the Glasgow Royal Infirmary.

[The writer of this paper has tried all the remedies or methods usually employed to prevent the pitting in cases of small-pox, and has found them each more or less objectionable. Solution of collodion certainly lessens the amount of pitting, but it often increases the irritation of the skin. Dr. Bell says:]

I have employed lard, oil, and the linimentum aquæ calcis, and found that if any of them were constantly applied, no irritation was produced, and that the pitting was very much modified. But as all these substances either run off the face soon or become dry, their benefit entirely depends on the assiduity with which their application is repeated.

Being favourably impressed with the effects of these, especially of the linimentum aquæ calcis, I was led to think that if it were applied on cotton wool, in the same manner as used in the treatment of burns, that the difficulty of keeping it applied might be obviated, and that the effect would be beneficial. Under these impressions, I applied it in several cases of confluent small-pox which were under treatment in the infirmary, immediately before the termination of my duties as physician to the fever hospital in 1854, and the result was so successful that, on my re-appointment in 1858, I employed it in every case of confluent small-pox with decided success. In some of the most severe cases of the disease, not a single pit was found to have been formed on the parts to which it was applied. At the point of the nose and around the mouth, where the patients contrived always to remove the dressing to some extent, numerous deep pits took place, and contrasted strongly with the uniform smoothness of the cheeks and other parts over which the dressing was permitted to remain undisturbed.

But, besides the effectual prevention of pitting, the dressing secured

another important result—*it prevented the swelling of the face.* This effect was most marked. In the severest confluent cases, if applied early, the swelling never attained any great extent; and in cases in which great swelling had occurred prior to admission, the tumefaction of face soon subsided after the use of the dressing.

The febrile symptoms became considerably mitigated after its application; indeed this effect was so striking, that I have been induced to regard the swelling of the integuments of the face and scalp as being in a great measure the cause of the high febrile excitement which continues to exist in the confluent form of the disease; but whether or not, I state the fact that in cases of severe confluent small-pox in which the application was employed, the swelling of the face, and also the febrile excitement, were so slight as to arrest the attention in the most decided manner.

The Mode of its Application.—The linimentum aquæ calcis should be poured on a plate; then masses of cotton wool, answering in size and shape to the parts to which the dressing is to be used, should be dipped in the liniment, and applied in such a manner as to completely cover the face and neck, leaving apertures for the eyes, nostrils, and mouth. The cotton should be closely matted together, so as to allow no crevice to exist, and a large handkerchief should be tied over all, having holes cut in it so as to correspond with the apertures over eyes, nostrils, and mouth. The dressing should be allowed to remain until convalescence, and if it becomes accidentally detached at any part it should be immediately renewed.

Dr. Stokes, in an excellent paper on the prevention of pitting, in the 'Dublin Journal' for February, 1859, speaks favourably of the advantages resulting from the linimentum aquæ calcis, but justly remarks that it rapidly becomes dry from the heat of the face. The use of the cotton wool, however, obviates this drawback, and at the same time completely secures the object which this distinguished physician states should be kept in view in the treatment, viz.:—

1st. The exclusion of air.

2nd. The moderation of the local irritation.

3rd. The keeping of the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

In order to effect these objects, Dr. Stokes has employed linseed-meal poultices with decided success. I feel assured, however, that the three objects mentioned will be much better attained by the method which I have now recommended, and, I am also inclined to say, with much more comfort to the patient. The linseed-meal poultice must cause uneasiness from its weight, and no small amount of discomfort from the smell which must be produced. There can be no question but that the cotton wool will secure both the exclusion of air and light, keep the parts in a moist condition, and, as I have stated, will reduce the local irritation, as is proved by the rapid subsidence of the swelling of the face after the use of the dressing. The

permanent nature of the application must be a great advantage over the frequency with which poultices require to be changed, as by this the air and light are often allowed to get access to the parts, and in this way detract from the value of any application which requires to be frequently changed.

The exclusion of air and light seems to be of the greatest moment; indeed it appears that if this can be accomplished by any plan, pitting will be prevented, or at least modified. It was on this principle that John of Gaddesden introduced the practice of blocking up the windows, and hanging scarlet cloth around the bed of the patient. In the 'American Journal of Medical Science' for 1832, Dr. Picton tells us that he had no cases of pitting from variola when the light was excluded from the room. In confirmation of these statements I may be allowed to remind the reader of the interesting experiments made by Serres. He placed a glass capsule over small-pox pustules, so as to keep out air and light; he ascertained that the pustule was rendered abortive in proportion to the exclusion of both, and that when both were completely excluded, the pustule rapidly dried up and left not the slightest scar behind. The same writer affirms that on one occasion he had to treat a number of small-pox patients in a kind of dark cellar attached to La Pitié, and that very little pitting took place in these cases.

It seems, therefore, that the exclusion of air and light will completely prevent the pitting in variola. I believe that no applications will be found more likely to secure these conditions than the one which I now recommend.—*Glasgow Med. Journal, July* 1861, *p.* 168.

74.—OBSERVATIONS ON THE TREATMENT OF DEFORMITIES RESULTING FROM SEVERE BURNS.

By Dr. John K. Barton, Surgeon to the Adelaide Hospital, &c., Dublin.

[The cicatrices left after severe burns produce, as is well known, much deformity. Dupuytren used to practice parallel incisions through these contractions, and afterwards an apparatus, which, by means of springs, would keep up a constant separation of the edges of the wounds until they had become healed. Subsequent operators have tried operations somewhat similar, but only with the result of repeated failures. Dr. Barton says:]

The causes of these unfavourable results from operations well planned and skilfully performed, will be found, I think, to arise from too much being expected from the operation, and too little attention being bestowed upon the subsequent process of extension or stretching; and that much more successful results will follow than have been hitherto obtained, by means of a careful and continued extension of the cicatrix, in some cases assisted by a cutting operation, such as has been mentioned,—in others, simply by subcutaneous sec-

tion of the unyielding bands, and frequently not requiring any assistance from the knife whatever.

A consideration of the cause of these deformities favours this view of the treatment. The cause of the forcible contraction of the cicatrix which produces the deformity is the lymph which is shed in the repair of the ulcers left after the separation of the sloughs, which, following an invariable law, as soon as it becomes a part of the organized tissue, slowly but forcibly contracts. This law we may observe in many parts of the body; for instance, when the lymph is shed over the pleura, we find the walls of the thorax yielding to its contraction; and when poured out in the capsule of Glisson, we know with what a powerful grasp it compresses the liver. And in stricture of the urethra, it is the same substance which, shed upon or beneath the mucous membrane, produces such a train of evils by its tendency to close the canal; and tries the patience and skill of the surgeon, in overcoming its constant tendency to contract. Now, the treatment of stricture of the urethra has, for more than 200 years, occupied the attention of surgeons both in this country and on the continent; and while there has been great diversity of opinion upon many points, I believe I am correct in saying that all the ablest surgeons who have written on the subject agree that, in the great majority of cases, dilatation alone is the safest and the most successful treatment that can be adopted: that in some the use of the knife may be required to obtain a passage, in the first instance, which then must be maintained by dilatation; and that the cases which admit an instrument at all, and will not yield to dilatation, are comparatively very few; that, in fact, a cutting operation is only occasionally required, while careful and patient dilatation is almost invariably sufficient, either in conjunction with operation or alone, to produce a successful issue. Now, the cause of the contraction in each case being identical, we may with great advantage use the experience which we have gained in the treatment of stricture of the urethra to guide us in the choice of means for overcoming the contraction of the cicatrices of burns, especially when cutting operations have been so freely tried; and when unassisted, or only partially assisted by extension, have failed to produce the desired result.

Whatever mode of treatment we adopt, our great object must be to obtain, if possible, the absorption of the lymph, which is the contracting power: if this be removed, the case is cured. Now, no cutting operating will, of itself, produce this desirable object; on the contrary, it will cause the effusion of more lymph, which, being quite recent, will no doubt be far more amenable to extension than the old cartilaginous lymph; and so cutting may help. But the means which we may trust to, to gain the removal of the cicatrized tissue is *extension;* and this, in some cases alone, in others aided by the knife, will, when perseveringly employed, produce the absorption of the tissues of the cicatrix, and so the permanent removal of the deformities dependent upon it.

That the plan of extension which I am advocating is carried out with success at the Orthopœdic Hospital, and elsewhere in London, would appear from the following passage, which is taken from the 'Lancet' of August 13, 1859. I have not been able to find any more complete account of the treatment adopted there, nor any report of cases since this :—

"*Simple Extension in Contraction from Burns.*—We lately had the opportunity of observing the treatment of a case of deformity, arising from an old burn, in a little boy, nine years of age, under Mr. Coote's care at St. Bartholomew's Hospital. It is a plan, I believe, in use in the Orthopœdic Hospital, and consists in the proper appication of simple extension perseveringly carried out. The boy was admitted on the 7th of April, with his lips and mouth drawn downwards from a burn in the neck when an infant. The cicatrix preserved the usual characters of hardness and thickening. By suitable appliances the head and chin were kept extended, with the effect of bringing back the lower lip and jaw to their natural position. and getting rid of the extreme deformity which had before existed. The mouth can now be closed. The effect of extension is to cause the absorption of the adventitious membraneous material present in the cicatrix, and thus permit the latter not only to become soft and extended, but permanently to remain so."

In the following case, it will be seen, I first performed James' operation, and then employed extension, aided by subcutaneous section. From the experience gained in the treatment of this case I would now *begin* with extension, carry it on perseveringly for some time ; then, if I found it necessary, employ subcutaneous section of the most resisting bands of the cicatrix, and finally resort to the more severe operation, if I found I could not succeed without it.

Case 1.—Isabella M'Owen, a healthy girl, fourteen years of age, from the County Meath, was admitted into the Adelaide Hospital, upon the 22nd of March, 1860. When about eight years of age, her clothes having caught fire, she was severely burnt about the right side of the neck and head. He mother had endeavoured to prevent the contraction which she was told would follow, by placing a stiff leather collar round her neck when the ulcers were healing, and for some time afterwards ; the contraction, however, went on increasing in spite of this, and, as the girl began to grow up, she became very much dissatisfied with her appearance, and urgently sought that something might be done for her relief. The cicatrix occupied the whole of the right side of the neck, its densest and thickest part being close beneath the ramus of the jaw, extending from the lower part of the ear, which was involved in it, to the chin ; from this central mass strong fibrous bands extended downwards below the clavicle, and as far forwards as the sternum ; superiorly, the skin of the whole of that side of the face was drawn down to it ; the angle of the mouth, and the external angle of the eye, being drawn downwards, the latter causing slight

ectropium. The head was kept bent down to the right side, and when held straight caused increased distortion of the countenance; but by no effort could the patient turn her head to the left side.

The girl and her parents being very anxious to have something done to relieve her deformity, I determined to attempt it, my colleagues having examined the case, and agreed with me, and having also had the advantage of Dr. Hutton's advice, who kindly gave me his opinion as to the best mode of proceeding.

Upon the morning of the 28th, as soon as the patient was well under the influence of chloroform, I proceeded to operate in the following manner:—An incision was first made along the posterior edge of the cicatrix, from the mastoid process to the acromion, then a second along the anterior or inner edge, from the chin to the sternum; thirdly, another incision was carried across the cicatrix just below its central mass, connecting the two former. The two flaps thus formed were then carefully dissected from their attachments to the parts beneath, which was a matter requiring both time and care, as the skin, platisma, and fascia were all matted together and to the muscles beneath, by the dense fibrous structure of the cicatrix. As soon as this had been satisfactorily accomplished, the deformity of the face was found to have disappeared, and a gaping wound remained, extending from the ramus of the jaw to the clavicle. No vessel required ligature. Lint wet in cold water was laid on the wound, and the patient removed to bed. Considerable constitutional irritation succeeded, which, being followed by an attack of bronchitis, reduced the patient very much, and prevented me applying any instrument for keeping her head in proper position for some time. I found some difficulty, also, in getting any instrument made which would fulfil the indication, viz., to keep the head in such a position that the cicatrization of the neck could not deform the face; at last I succeeded in getting an instrument made by Read, which has answered so well, that with modifications, I think it will be found applicable to all cases of contraction about the neck or face. It consists of a shoulder-piece which, before the steel of which it is made was hardened, was fitted over the shoulder so as to sit closely and firmly; this was fixed in its place by two straps passing round the chest; from its upper side projected two steel slips, moving upon two others, which connected it with a firm cushion, which fitted against the ramus of the lower jaw, and was fixed firmly there by two straps. When the shoulder-piece and cushion were firmly strapped in their places, the head was forcibly drawn over to the left side, thus putting the parts between the jaw and the clavicle very much on the stretch, the two pairs of steel slips sliding upon one another; the screws being then turned, the apparatus was fixed, and remained so the whole day: it was taken off at night, and reapplied, carefully, every morning. Recontraction was very rapidly taking place when this instrument was first applied; the bands of lymph in the cicatrix were extremely strong and unyielding,

and the face was again being drawn into deformity; so that, from the first, the work this extending apparatus had to accomplish was to stretch the cicatrix, and thus cause its absorption. It became a matter of great interest to me to see if it could perform this; it was therefore carefully put on every day, for about three months. When about one month had elapsed, I saw some progress was slowly, but steadily, being made, and to aid the progress going on, I now divided subcutaneously, two or three of the most resisting bands: this was decidedly of service, so I repeated this about every fortnight, until at the end of three months the thickness of the cicatrix was very much decreased; it was evident that the dense fibrous tissue was being removed, and that the steady extension, aided by the subcutaneous section of the bands, was producing an absorption of the lymph of the cicatrix; a corresponding improvement had taken place in the deformity of the face; the eye was quite free, while the corner of the mouth was very slightly pulled down; the head, too, could be turned to the left side with freedom, and the face turned round to the left side completely; but when this latter motion was performed, the mouth and cheek were still a good deal dragged, The patient now went to the country,—her mother having learned in the hospital, in a day or two, how to put on the instrument, and undertaking to apply it daily, and bring back the girl in November, which she did, having been absent about two months. Her neck remained very much in the same state when she returned as that in which it was when she left the hospital, but her general health was much improved. No doubt the apparatus was not kept on as regularly, nor applied as firmly, as it should have been; upon her re-admission, however, it was again carefully put on, the most prominent bands in the cicatrix being again divided with the tenotomy knife, and a progressive improvement took place; the skin being soft and pliant where it had been hard and puckered, the deformity of the face disappearing at the same time. All this, however, took place very gradually. She was still under treatment in March, 1861, when, on account of the death of a sister in the country, she suddenly left the hospital. Her parents did not wish anything further to be done, expressing themselves very much pleased with the improvement which had taken place.

In this case, as in most others of a similar kind, the extending apparatus was put on at first with the object of keeping the edges of the wound separated while cicatrization went on; but very soon I found it would have to accomplish much more than this: if I was to obtain a favourable result, it would have to produce the absorption, to a great extent, of the old lymph of the burn and the new lymph of the wound, which together were rapidly reproducing the deformity. This it accomplished after being steadily and perseveringly employed for six months, aided, during a part of this time, by a subcutaneous division of the most resisting bands.

In proceeding to treat any case of this kind by extension, it will

be necessary to have an instrument which will fulfil the indication, viz., to keep the cicatrix constantly and firmly extended. The difficulty of accomplishing this, particularly when the neck or face are the parts to be operated upon, is one cause why so little has been accomplished by this means. The instrument I used in this case I found to answer very well; it is very simple, and, when properly put on, it remained *in situ* without slipping, and kept up very powerful extension on the cicatrix for twelve hours daily. In putting it on, care must be taken that the lower piece is firmly fixed to the shoulder by the straps attached to it, and also that the cushion is firmly fixed against the face by the straps going round the head, before extension is made; by attending to this, an intelligent servant, or relative of the patient, will readily learn to fix it on daily. After a few days, in the case I have related, it gave the patient but little annoyance, habit and the hope of a cure quickly reconciling her to its use. An instrument made upon the same principle, viz., a lower piece resting upon some part where it can be firmly fixed, connected to an upper movable cushion by slips sliding on one another, and fixed by screws, can be readily adapted for each particular case.

An objection which may be fairly urged against the treatment by simple extension, is the length of time it takes to produce the desired result. Now, in a case where the thickness and extent of the cicatrix is great, no doubt it will be a very tedious process; in such cases I would recommend the adoption of subcutaneous section of the most resisting bands of the cicatrix; I found it a valuable auxiliary in the cases I have related. I generally divided two or three of the most resisting bands at one time; in some cases the skin over the dense tissues divided, being very thin, and involved in it, either gave way or were cut through; but in no case was there any constitutional disturbance, and only a few drops of blood were lost each time, while undoubtedly the extension was very much facilitated by it.

While the majority of cases will be most successfully treated, I believe, by simple, persevering extension, aided, when the cicatrix is situated in the neck, by subcutaneous section, yet there are some cases, where the cicatrix is very large and the mass of organised lymph very great, which will not yield to these means, just as there are some cases of stricture of the urethra which will not yield to dilatation, but demand, on account of their dense cartilaginous character, some other measures. In such cases, after having given a fair trial to simple extension, I would then operate, and as soon as possible put on the extension-apparatus again. As to the mode of operating, the proceeding adopted in M'Owen's case, first recommended by James, will frequently answer very well; or a V-shaped incision may be made both above and below the cicatrix, *through the healthy skin, close to the cicatrix, but not through it*, which may then be dissected from its attachment sufficiently to admit of extension to the desired extent; this plan I have seen successfully practised by my colleague, Dr. Walsh.

Besides those cases of extensive and cartilaginous cicatrices which we must seek to modify by an operation before we can successfully employ extension, there is another set of cases which will be advantageously treated by a cutting operation in the first instance; these are cases where a web has been allowed to form between adjacent parts, such as between the fingers or toes, or between the arm and side, by which the motions of the parts are crippled, if not destroyed. In such cases we can remove by the knife a great part of the contractile tissue, and yet, as the parts will allow of being drawn together without any deformity resulting, leave a very small surface for granulation, and by subsequent extension we can prevent the re-formation of the web. The following case, which is still under observation, exemplifies well this kind of cicatrix, and the mode of its removal.

Case 2.—Mary Kelly, ten years of age, a healthy-looking little girl, was brought up to the Adelaide Hospital, from the neighbourhood of Navan, upon the 13th of April last, for the purpose of having something done to restore, if possible, the motions of her right arm, which was connected to her side by a very strong cicatrix, which rendered the limb almost useless. Her mother stated that when the child was only four years of age her clothes caught fire, and she was very severely burnt along her right side and arm. She was attended by a doctor; but, after he had ceased to visit her, the child remained quite unable to move her arm from her side, on account of the pain which any motion of the limb gave her; the sores remained open for nearly a year; as they were gradually healing, she observed that the arm was contracted to the side, and could scarcely be moved. Soon after this she brought the child to Dublin, to see what could be done for her. She saw Dr. M'Dowel at the Whitworth Hospital, and was advised by him to return to the country for another year, at the end of which time, if she brought back the child, he would be able to do something for her relief; in the mean time the arm was to be exercised as much as possible. No doubt Dr. M'Dowel remembered the advice given by Dupuytren, not to operate upon the cicatrices of burns until they had fully contracted. Various circumstances occurred to prevent her bringing the child at the time named, and she allowed five years to elapse, until, being urged by her friends to have something done, she came to the Adelaide Hospital.

The cicatrix was both very extensive and thick; it completely filled up the axilla, extending two inches below the bend of the elbow on the outside, and as far as the last rib on the inside; it was as thick as the fore-finger, and very dense. The motions of the arm were, as may be supposed, greatly interfered with: in the first place, it could not be separated from the side for more than six inches, measured from the elbow to the chest; the backward and forward motions were very much limited; and the fore-arm could not be straightened on the arm. The muscles of the arm and fore-arm were very much atro-

phied, but the bones had attained the same size as the opposite limb. The artery retained its normal position.

I determined in this case, before applying extension, to perform a cutting operation, for the following reasons:—1st. The density and unyielding character of the cicatrix made it doubtful whether it would yield to extension alone, and certain that it would be a very long and tedious process: 2nd. The peculiar position of the cicatrix made it quite possible to remove a great portion of the contractile tissue altogether, leaving at the same time only a small surface to heal by granulation.

Accordingly, upon the 24th of April, the child being under the influence of chloroform, I proceeded in the following manner:—An incision was first made along the outside of the cicatrix and inside of the arm, extending from the top of the axilla to about three inches below the elbow-joint; the knife was then turned round, and carried from the last-named point along the free edge of the cicatrix to the side of the thorax, when a third incision along the inside of the cicatrix was made from the point where the first began to where the second terminated; thus a triangular piece was included, which was the whole anterior surface of the cicatrix; this was now rapidly dissected from the posterior part, and removed; the posterior layer of the cicatrix was then divided perpendicularly from above downwards, its external part brought over the arm, and its internal over the chest, and the edges then drawn together by seven or eight points of twisted suture, and well supported by broad strips of adhesive plaster; wet lint was laid over all, and the child removed to bed. Some of the needles were removed upon the third day; two were allowed to remain, where there was much strain, for two days more. When these were withdrawn, the lips of the wound retracted to some extent, so that the surface to be healed by granulation was, upon the side of the chest, two inches in width and six in length; upon the arm it extended from the axilla to three inches below the elbow, but was not more than an inch and a half broad anywhere. At the apex of the axilla there remained, however, a surface fully four inches across for granulation; the extent of this surface rendered the secondary treatment of keeping the parts extended while granulating very important.

The apparatus I employed for this purpose consisted of two parts—1st, a simple elbow-splint, which, being fixed to the arm and fore-arm when bent, could be strengthened and kept at any angle by a screw; by means of this, in a day or two, the fore-arm was brought into a straight line with the arm, and kept so during the rest of the treatment: 2nd, to keep the arm from the side, and thus prevent the re-forming of a cicatrix between the parts, I adopted the following plan: a broad well-padded girdle was strapped round the child's waist, resting against the pelvis, from which, directly below the axilla, and united to it by a hinge-joint, extended a steel rod, about two feet and

a half in length, curved so that a rest for the arm could, as the arm was raised from the side, easily slide along it; by means of a screw, the rest could be fixed at any part of the rod, and thus the elevation of the arm easily regulated and maintained at any point. I found this instrument work remarkably well; the only change it became necessary to make in it was to have the handle of the screw, which fixed the position of the rest on the rod, filed off, and a key made to turn it, which being in the custody of the dresser who had charge of the case, put it out of the power of the little patient or the nurse to let down the arm after it was fixed. As soon as the needles were removed, this instrument was put on, and the arm held at right angles to the chest, and the fore-arm in a straight line with the arm. Both night and day the instrument was kept on, causing the patient no annoyance after being carefully fixed in the morning. As soon as granulation had fairly set in, I found it necessary to raise the arm higher than a right angle from the side, so strong was the contracting power, and also to repress the granulations in the axilla, which had a tendency to grow down rapidly, by the frequent application of nitrate of silver, and by tightly strapping the shoulder with plaster. This tendency of the granulating surface to extend downward, while at the same time contracting so as to draw the arm to the side, explained what, when I first saw the case, seemed very hard to comprehend,—how such a dense mass of tissue could have been formed between the arm and the chest. The connecting tissue grew from above downwards, uniting, as it extended, the granulating surface upon the chest to that upon the arm. This case is still under treatment (June 28); and although it is more than two months since the operation, a great part of the granulating surface is yet unhealed. This excessive slowness arises from the position in which the arm is necessarily maintained, which position will I believe have to be maintained more or less for the next three months, before the danger of recontraction will be over. The motions of both elbow and shoulder are now nearly entirely regained, passive motion being employed daily.

This was, without doubt, a case in which much was gained by commencing with a cutting operation; yet the struggle only began with this, which was the easiest part of the treatment; and the final result —the restoration of the motions of the right arm—depends more upon the less brilliant subsequent treatment.

All cases of deformity produced by extensive cicatrices are serious cases; and the surgeon will do well to bear in mind, when undertaking their treatment, the obstinately contractile character of the morbid tissue against which he is about to proceed; that to obtain the removal of this tissue must be the object of his treatment; that in no case can this be obtained by a cutting operation alone; that extension, if patiently and perseveringly employed, will cause the absorption of this tissue; and that, therefore, how this can be efficiently employed in any particular case, must first occupy his attention. Then he may

consider in what way this process may be facilitated; and here the knife will furnish him with the aid he requires—thus considered as an aid to extension, there will be no disappointment to surgeon or patient at the result of the operation. And when performed after and during efficient extension, it will be far more successful than it is found to be when performed first, and as the principal part of the treatment.

In these observations I have scarcely alluded to the plastic operation, because it is unsuited to the kind of cases I have been considering, viz., extensive and deep cicatrices. In cases where a flap of healthy skin could be obtained, and where the cicatrix was so small that there would be no likelihood of sloughing of the transplanted piece, such as a narrow cicatrix of the cheek, causing ectropium, I would prefer this mode of proceeding to any other, and have seen it very successfully adopted.—*Dublin Quarterly Journal, Aug.*, 1861, *p.* 1.

75.—TREATMENT OF SCABIES BY BENZINE.

By J. J. Godfrey, Esq., Malvern.

That sulphur, though a certain, is by no means a satisfactory specific for scabies, is evidenced by the appearance of statements, from time to time, of the success of other applications; and, notwithstanding Professor Hebra's assertion that "all methods of treatment which do not include sulphur are more or less uncertain," it is well known that the itch-insect can be destroyed by several other agents, such as iodide of potassium, creasote, &c. That will be the best remedy, which is easiest of application and quickest in action. Dr. Metzel employs phosphorated oil; which, however, like sulphur, requires several applications.

I wish to draw attention to a remedy which I have seen recommended; but which I believe has not been largely tried, at least in England, and which appears to be almost instantaneous in its action; I refer to benzine. M. Bonnet in 1858, noticed this as a specific for itch, the cure being effected in a few minutes. M. Rey also speaks of cutaneous insects being destroyed by benzine, and suggests it; employment in scabies. During the last five months, I have used benzine in seven cases of itch, and with perfect success; one application having proved sufficient. Three cases (one very severe) occurred in the same family. Three ounces of the fluid were supplied, with directions to sponge every part of the body with it where any rash or itching was noticed. This was done in the evening, and the whole was used. The application was not repeated; and after a month there had been no return of the disease. The number of my cases is so few to draw any strong conclusion from, that I should be glad to know with what success it has been used by others. Should benzine prove constantly, or even frequently successful, its application will, to say the least, be a pleasanter mode of dislodging the acarus scabiei, than bedaubing the body with sulphur ointment.—*British Medical Journal, Sep.* 28, 1861, *p.* 343.

76.—ON THE TREATMENT OF PHTHIRIASIS.

By Dr. T. McCall Anderson, Fellow of the Faculty of Physicians and Surgeons, Glasgow.

The treatment of phthiriasis is abundantly simple. Many are the medicines and varied the forms which are used for this purpose. The preparations of sulphur, mercury, staphisagria, sabadilla, pyrethrum, the essential oils and alcohol, are principally employed.

Sulphur is used in the form of vapour-baths or fumigations, or the simple sulphur ointment of the pharmacopœia may be employed.

Mercury may be applied in the form of simple mercurial ointment, or fumigations with cinnabar, or a lotion of the bichloride in the proportion of two or three grains to an ounce of water, the solution being facilitated by a few drops of alcohol.

Staphisagria in powder is a very good preparation, or an ointment made by mixing an ounce of staphisagria with four ounces of lard and a few drops of an essential oil, as the oil of rosemary, to improve the odour. Or an infusion of staphisagria may be made with vinegar.

Sabadilla is employed either in powder or ointment, the latter consisting of a drachm of sabadilla to an ounce of lard, and scented as above.

Pyrethrum is generally applied in powder. Any of the essential oils may be used, the oil of rosemary being generally selected.

In the case of lice attacking the head, the hair should be cut short, if it is long and entangled—especially if the scalp is the seat of secreting eruptions, as we are thus enabled to remove many of the nits adhering to the hair, and to get more readily at the pediculi, A lotion of the bichloride of mercury, or the powder or ointment of staphisagria, or simple lard or oil should be applied, which causes the death of the insect. It is then necessary to remove the nits, which may be done by applying repeatedly either alcohol or dilute acetic acid, as Hebra recommends. These, after the use of the spirit, cease to adhere to the hair and may then be removed by combing. The head should also be repeatedly washed in warm water with soap. The eruptions of the scalp left after the removal of the insects must be treated according to their nature.

There is just one remark which remains to be made with regard to the treatment of pediculi capitis,—one which has been very prominently brought forward by Devergie,—and it is this,—that, in those cases where the head has *long* been attacked by *great numbers* of pediculi, especially in the case of children, it is dangerous to eradicate them suddenly, for by so doing internal diseases have been developed; and Devergie has seen two infants who died from this sudden cutting short of the accustomed itching and secretion. It is, therefore, necessary, in such instances, to cure the disease more slowly, to attack only small portions of the head at a time, and especially to avoid cutting short

the hair at once. It is only in very rare instances, however, that such precautions are required.

The pediculus corporis is best attacked by sulphur vapour-baths, or mercurial fumigations, or the powder of staphisagria or pyrethrum, If one of these powders is used, warm-baths should be superadded. The clothes should be purified by exposing them to a temperature of 150° Fahrenheit, or as the common people do sometimes, and which Küchenmeister has seen successful, by burying them in hay for several weeks. Washing clothes may be put into boiling water, or cloth clothes exposed to the steam rising from a boiler. The pruriginous eruption which the lice have occasioned will disappear after they have been killed; but any constitutional eruption, such as eczema, which may have been called forth by the irritation of the scratching, or which existed previous to the occurrence of the lice, must be treated on general principles. When an eczema exists, it may sometimes be necessary to treat it first of all (on the same principle as an ordinary eczema) before attacking the pediculi; for they are very apt to lie hidden among the crusts, and soon multiply again, giving as much annoyance as before. This remark is applicable to all the forms of phthiriasis.

The pediculus pubis is easily killed by rubbing into the *roots* of the hair a lotion of bichloride of mercury, which is invariably effectual, provided it be brought into *direct contact* with *all* of them. The application should be made, not only to the hair of the pubis, but also to the hair about the scrotum, perineum, and anus, which should be repeated daily for a week. But if any excoriation exists, care must be taken not to use the lotion too freely, else salivation may be induced. In cases where there are many excoriations or abrasions, it is safer to use an ointment of sulphur or staphisagria, or an infusion of the latter, or the oil of rosemary. Mercurial ointment is the preparation most in vogue for this complaint, but it is a dirty application, and I never employ it.

There are very many other remedies which have been successfully used, but is quite unnecessary to allude to them further, for those already mentioned are amply sufficient, when properly used, to kill the pediculi, no matter how numerous they may be.—*Med. Times and Gazette, June* 8, 1861, *p.* 598.

77.—ON THE TREATMENT OF ECZEMA.

By Prof. Hebra.

One has the satisfaction, Professor Hebra observes in his clinical lecture, of knowing that we can always cure eczema, however long it may have continued, although in some inveterate cases depending upon a dyscrasis, or upon internal causes, there may be considerable difficulties in the way. As in most other diseases of the skin, the treatment should be purely local, internal measures being limited to the rare cases in which the eczema has been produced by a previously diseased

condition of the economy, or when it is combined with some other affection. In the great bulk of cases internal means, such as mercury, antimony, iodine, purgatives, sarsaparilla, &c., are superfluous, and mischievous. Arsenic is the only one of such means which exerts any influence in obstinate cases. Ordinarily, however, it is of no use, and its employment should be limited to the few cases which manifest especial obstinacy, the local treatment in these also not being at the same time neglected.

Cold water, in its various modes of application, is of great importance, combined with other means, in the treatment of eczema. Employed alone it is far too tedious, and frequently not sufficing in its operation. It only aggravates the malady when applied in *eczema simplex*, arising from excess in secretion, as *e.g.* in the axilla, between the buttocks, &c. Starch, whether alone, or mixed with oxide of zinc (starch ℥j, zinc ℥iij) is, on the other hand, an excellent application when the eczema arises from the friction of two cutaneous surfaces, or from excessive secretion, as in the axilla, under the breasts, the scrotum, buttocks, &c. Oxide of zinc (℥j, to ℥j of lard), sulphate or acetate of zinc, alum (℥j to ℔j of water), red or white precipitate (6 to 12 grains to ℥ij of lard), are of good service in acute eczema or in chronic when there is but slight infiltration and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost indispensable *schmierseife*, by reason of its slightly caustic action, becomes the most preferable remedy. Its mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar.

In many chronic cases, with great infiltration, the soap does not suffice, and we must have recourse to a stronger form of potash, viz., caustic potash, 1 drachm dissolved in 2 drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. These are then washed, and cold applications are kept constantly employed. One or two such cauterizations usually suffice, and when more are necessary they should not be repeated oftener than once a week. Carefully applied, the caustic gives rise to no cicatrix, but the practitioner must himself always undertake its application. It is a very painful procedure, and is not often required.

Cauterizing with strong acids is to be avoided, as it causes great pain and gives rise to scars. The application of the nitrate of silver is of no avail. *Tar* is in its way just as useful a remedy as the soap, the time for its application being when the moisture and itching have ceased, and exfoliation has commenced, *i.e.* when *eczema squamosum* is present. It may be used either alone or mixed with equal parts of cod-liver oil, and should be applied by means of a brush once or twice a day, carefully avoiding washing the parts or allowing water to come into contact with them. As long as any redness or desquamation continues, the tar must be repeated. Sometimes, when the application of the tar has been premature, moisture and itching are observed at certain spots, and the preliminary treatment has then to be resorted to again. Some individuals cannot bear the tar at all, it giving rise to severe inflammatory action. An ointment of acetate of lead or oxide of zinc should in such cases be substituted. *Cod-liver oil* is a valuable external application, and by its aid alone we are able to cure the eczema when this has not lasted very long and the infiltration is not very considerable. It is also an excellent adjuvatory to the treatment by *schmierseife* and cold applications, as flannels soaked in the oil may be kept bound over the diseased parts during the night. Employed alone, the treatment is very tedious, and is objectionable on account of the disagreeable smell and befouling the linen which it gives rise to. Taken internally it does not exert the slightest influence on eczema.—*Wiener Spitalszeitung, and Glasgow Med. Journal, April* 1861, *p.* 107.

SYPHILITIC AFFECTIONS.

78.—ON SYPHILIS.

By Henry Lee, Esq., Surgeon to the Lock Hospital.

If there be one pathological fact that has been more frequently reiterated of late years than another, it is, that a primary syphilitic sore may be distinguished from every other affection by its secretion when inoculated, invariably producing the charateristic syphilitic pustule. Although syphilitic inoculation was practised by Hunter and other accurate observers, it remained for Ricord to draw well-digested conclusions from the facts which this mode of investigation presented for our interpretation. By patient experiment and rigorous reasoning Ricord established the truth, which has now been confirmed throughout Europe from Italy to Norway, namely, that certain forms of syphilitic sores will produce, when inoculated, their like, which in their turn will again furnish a secretion capable of being again inoculated with similar results. It would be difficult to overrate the importance of this discovery, so analogous to that of the small-pox inoculation. But practical men have, nevertheless, been unable to see in the experiments in question a conclusive representation of that which they

daily observe in practice. That certain forms of primary syphilitic disease will produce the characteristic pustule, when inoculated, all must allow; but it does not follow, on the one hand, that all primary syphilitic sores are alike capable of being inoculated; nor, on the other, that when inoculated, they will necessarily yield the same result. Under these circumstances, and without proof as to these two last mentioned particulars, to say that a sore is syphilitic because it produces a characteristic pustule when inoculated, and that it produces a charateristic pustule because it is syphilitic, is manifestly to reason in a vicious circle. The propositions themselves are undoubtedly true; but they cannot, without further proof, be received as excluding other modes of syphilitic infection and other forms of primary syphilis.

We are led then to ask, in the first place, whether there is no form of primary syphilis except that which commences as a pustule. This question is important, since surgeons engaged in practice very seldom have an opportunity of observing the primary syphilitic pustule; and still less frequently have they an opportunity of tracing any secondary symptoms consequent upon it. Now, during the years 1855 and 1856 we examined at this hospital the secretion from a large number of primary syphilitic sores, and amongst these were ninety-five cases presenting all the ordinary characters of the primary infecting sores. The secretions from these ninety-five cases consisted of epithelial *débris* floating in a serous fluid, or of globules of various shapes and sizes, which did not, upon the addition of acetic acid, yield the well-defined nuclei characteristic of pus. In a very considerable number of cases the secretion appeared to consist of nothing but epithelial *débris* and serous fluid, and in some the sores yielded no fluid at all.

What reason, then, have we to believe that these primary sores commenced as pustules; or that, unless artificially irritated, they would produce pus at all? And, secondly, what evidence have we that every syphilitic sore produces a secretion capable of being inoculated in the same manner? It is to this point that I wish principally to devote this lecture.

When the secretion is taken from a chancre during its period of progress, or when stationary, and properly inoculated, the following results may, according to M. Ricord, be invariably observed. During the first twenty-four hours, the inoculated point becomes red; from the fourth to the fifth day, the secretion becomes purulent; and a pustule, with a depression in its centre resembling those of small-pox, is fully formed. ('Traité Pratique,' p. 89.) The inoculation never fails when properly performed. "*L'inoculation n'échoue jamais quand on prend le pus dans les conditions voulues et qu'on l'applique bien*" (p. 94). The appearance produced is said to be regular, characteristic, and uniform (p. 135). It is useless to multiply quotations upon this point, the facts concerning which have been so generally received and so often republished.

In the year 1856, I published an account of some cases in which, notwithstanding the assertions so unreservedly made and so generally received, I had not been able to produce any effect by ordinary inoculation. The experiments were tried in this hospital, care being taken to select instances in which the sores had not begun to heal; and among the cases were the following.

Thomas C., aged 16, was admitted on the 27th of November, with an indurated sore extending half-way round the margin of the prepuce, causing phimosis. This had commenced a fortnight previously. The secretion from the sore was carefully inoculated on the patient's thigh in several points. The inoculations were followed by no result, and the patient was soon afterwards affected with secondary syphilis.

James G., aged 25, had a large indurated sore near the orifice of the prepuce, which had appeared as a pimple four weeks previously. The secretion was carefully inoculated in several points, but without result. Constitutional syphilis followed.

Matilda P., presented on the left external labium a well marked indurated sore, which she stated had existed one week only. The secretion, which consisted of a thin serous fluid, was carefully inoculated. No result followed the inoculation.

Julia B., aged 21, had a red glazed sore on the external labium, surrounded by distinct specific induration. The disease had commenced three weeks previously, as a pimple. The secretion from the surface of the sore was inoculated without result.

Since the period above referred to, we have inoculated the secretion from a considerable number of sores presenting the characters of the specific adhesive inflammation; and, as a rule, no result has been obtained where no artificial irritation has been applied. The secretion in these cases has consisted chiefly of epithelial *débris* floating in serum more or less turbid. The character of the secretion of these sores may readily be altered by anything that is brought in contact with them. The application of caustic, or a thick scab which confines the secretion, or a piece of linen which sticks to the sore, will produce a temporary discharge more or less puriform; but dress the part with wet lint for a day or two, and the natural character of the discharge will again be evident.

As a rule, then, I say (at least, that has been our experience at this hospital) that this kind of primary sore is not, under ordinary circumstances, capable of being inoculated with the lancet; and we therefore cannot but come to the conclusion that those who have maintained that all primary syphilitic sores can be alike inoculated have generalised too hastily. But a much more important question remains; namely, Does it follow that, because these sores are not capable of being inoculated by the lancet in their ordinary condition, therefore they cannot be communicated at all? And, if capable of being inoculated under certain circumstances, are the results produced identical with those produced by the inoculation of the secretion from suppurating sores?

In order to determine these questions, I would request your particular attention to some cases now in the hospital, and particularly to that of a boy named A. The details of this case, and the results of the inoculations performed, I will immediately mention; but, before doing so, I would draw your attention to the fact that in practice we continually meet with cases which in their early stages are similar to the one I am about to describe, and which, with or without treatment, become covered by epithelium; and, although the specific induration remains, yet the surface of these sores affords no fluid secretion whatever. Inoculation in the ordinary way at this period of the disease would be entirely out of the question. But, if a person in the condition I have mentioned get married, his wife will have a good chance, even before her pregnancy, of becoming infected with syphilis. Some very distinct cases where this has happened have fallen under my own observation. How, then, is the disease communicated in these instances? We have here a rather numerous class of cases in which the sores have become covered over with cuticle, which yield no pus nor fluid secretion of any kind and yet which are capable of communicating infection from one individual to another. How, I ask, does the inoculation take place from a sore affected only with the specific adhesive inflammation either before or after that sore has apparently healed? Some light will I think, be thrown upon these questions by the careful consideration of the facts presented to our notice in the boy A., at present in the hospital.

This lad had had gonorrhœa six months before his present attack, but otherwise he had never had any venereal symptoms. His present disease was of about a fortnight's duration. He first perceived a superficial sore behind the corona glandis, which healed in a few days. Two or three days after the first appearance of this sore, a little pimple appeared on the outer skin of the prepuce. This was squeezed, and discharged a watery fluid. A circular sore then formed, which continued to increase. He applied to me on the 25th of July, with a well marked Hunterian chancre. This was of a circular form, surrounded by well marked and accurately defined induration, and discharged a white turbid secretion. This fluid was placed under the microscope, having previously been mixed with a little acetic acid. It contained no pus-globules. The glands in the groin were enlarged and indurated, but not inflamed.

July 27th. The secretion from the sore was inoculated in several points on the patient's thigh.

July 29th. The boy was admitted into the Lock Hospital. The inoculations had been followed by no result. The secretion from the sore was again examined, and found to contain no pus.

July 31st. The sore, which continued to increase in size, had been dressed with linen and cold water since last report. The linen appeared to have irritated the sore in some degree, and numerous globules now appeared in the secretion; but, upon the addition of

acetic acid, the distinct outline of the pus-nuclei were not visible. Several fresh inoculations were made. The sore was dressed with wet lint.

Aug. 3rd. None of the inoculations had succeeded. The secretion from the surface of the sore, placed under the microscope and treated with acetic acid, did not appear to contain any pus. The sore was now ordered to be dressed with the acetum lyttæ. The glands at the back of the neck were now enlarged, and skin presented for the first time the appearance of a syphilitic eruption.

Aug. 5th. None of the former inoculations had been followed by any effect. The application of the acetum lyttæ had produced a superficial slough on the surface of the sore, and blistered the surrounding skin. Some distinct pus-globules were now visible in the secretion from beneath the slough. This secretion was inoculated upon the thigh in several points.

Aug. 7th. The sore now again secreted no pus. Fresh inoculations were performed.

Aug. 10th. The sore was dressed twice yesterday with the unguentum sabinæ, and it now yielded a copious secretion of pus. This was inoculated in several points in a fresh place upon the thigh.

Aug. 12th. The inoculations last made had succeeded. The primary sore still yielded a copious secretion of pus.

Aug. 14th. The sore had been dressed with lint, kept wet with water since last report, and now no pus could be detected in the secretion. The inoculations both of the 5th and the 10th had now succeeded. They presented the appearance of circular red patches, with some elevation and thickening of the cuticle. In one place there was the appearance of a broken vesicle, from which a serous secretion exuded. This secretion from the inoculation was again inoculated on the thigh.

Aug. 17th. The inoculation from the inoculation had succeeded. It presented the appearance of a red circular patch, from which the cuticle was abraded, with slight thickening of the skin. It had not at all assumed the appearance of a pustule, nor was anything like pus secreted from its surface. A single pustule, surrounded with very little inflammation, had formed in one of the points first inoculated. The eruption on the surface of the skin was fading. All the inoculations in this case were made with a lancet used for no other purpose, kept carefully wiped and wrapped in paper.

Aug. 19th. The inoculations appeared as separate red patches on the skin. The skin in these situations was slightly raised and thickened, but no induration extended into its substance. The solitary pustule which had appeared had dried up. The original sore was healing; but, being dressed with the unguentum sabinæ, it afforded a purulent secretion, which was inoculated upon another patient who was apparently suffering from organic disease of the liver.

Aug. 21st. The inoculations presented the same appearance as before; they appeared covered in certain parts with thin scales. They were tender to the touch, with slight thickening on the surface, which did not extend into the substance of the cutis. The inoculations nowhere presented any appearance of ulceration. The inoculations performed on the 19th had produced a small dark brown circular patch, in which the skin was slightly elevated. The secretion from the original sore was now again inoculated on a second female already affected with constitutional syphilis.

Aug. 24th. One of the inoculations on the boy had a slight tendency to ulcerate; the others were desquamating and losing their colour.

The single inoculation on the first woman had assumed the form of a small red pimple. The redness gradually faded into the colour of the surrounding skin. The cuticle at the inoculated part was thickened and elevated.

The inoculation on the second woman, already affected with constitutional syphilis, was less marked. There was only a slight redness and elevation at the inoculated part.

The particulars of these last two cases we may follow out on a future occasion. From the facts which have already been brought under notice, the following very important points are proved.

1. That some primary syphilitic sores cannot readily be inoculated in the ordinary way upon the patient who has them, either during the period of progress or afterwards.

2. That the sores which are not capable of being thus inoculated, as far as we have hitherto seen, are those affected with specific adhesive inflammation, and which do not, except under conditions of artificial irritation, secrete pus.

3. That these sores, although not capable of being inoculated in the ordinary way, will nevertheless, when irritated, furnish a secretion which is capable of being inoculated upon a patient already affected with syphilis.

4. That the inoculations thus produced do not give rise, as a rule, to either suppuration or ulceration, but to some adhesive form of inflammation.

The inoculations to which attention has now been directed, and the results of which may be seen in two other patients now in the hospital besides those to whom I have above referred, have all assumed a remarkably uniform character. There are many points of the greatest interest connected with this newly demonstrated form of artificial inoculation, which must be carefully distinguished from the effects of inoculation upon a patient not previously diseased. Here, after a certain period of incubation, a well marked Hunterian chancre would probably have been the result of each inoculation performed for the first time.—*British Medical Journal, April* 20, 1861, *p.* 407.

79.—TREATMENT OF GONORRHŒA BY INJECTIONS OF LIQUOR HYDRARGYRI PERNITRATIS.

By G. Borlase Childs, Esq., Surgeon to the Metropolitan Free Hospital.

The following report has been furnished us by Dr. John Warner, resident medical officer:

The effects of the above remedy, employed by Dr. G. Borlase Childs in the treatment of gonorrhœa, have been watched with much interest. In laying his experience of this remedy before the profession, Mr. Childs observes: "Gonorrhœa is a disease so common in large cities, and so well understood by medical men generally, that any further observations on the subject may be regarded as not only unnecessary, but altogether unacceptable and tedious. There is no disease, perhaps, which affords so wide a field for fancy treatment, and there are few practitioners who are not wedded to some peculiar treatment of their own. Whilst some rigidly pursue the antiphlogistic treatment, others discard it altogether, and strive to arrest the disease by the immediate exhibition of those remedies which are generally regarded as exercising a specific control over it. In both cases the unfortunate patient is dosed *usque ad nauseum*. The functions of the stomach become impaired, the general health suffers, and, though the acute character of the symptoms is subdued, the patient is frequently left with a troublesome gleet, aggravated and prolonged by the continued use of copaiba and cubebs. Such cases are of common occurrence, and the result of my experience leads me to the opinion that chronic discharges from the urethra are oftener the result of injudicious treatment than of the disease itself. With some practitioners there appears to be a deep-rooted prejudice against any other than a constitutional treatment, and the pertinacity with which they adhere to it is something marvellous. Local treatment is the exception and not the rule; and whilst in most other local diseases local remedies are employed, in the early stages of this they are rarely entertained. Surely the facility for applying local remedies to the urethra is not less than to any other inlet or outlet of the body. The specific inflammation, as described by Hunter, does not extend beyond four inches from the meatus, and therefore can be easily reached. It cannot be from any obstacle of this kind that local remedies are so frequently neglected: we must look for some other cause, and I think it will be found in the groundless fear entertained that injections either induce orchitis or lead the way to stricture. Now this I am prepared to deny, and my experience leads me to the conclusion that a judicious and well-timed employment of injections cannot fail to lessen the risk of such complications. Orchitis in the earlier stages of gonorrhœa is rarely met with—not until a week or ten days have elapsed, and when the inflammation has crept surreptitiously along the passage—when, in fact, it has passed beyond its specific boundary.

The same reasoning applies to stricture, with this exception, that the former is the result of acute, the latter of chronic, inflammation. Inflammatory stricture I have never seen induced by injections. Such being granted, it is not unreasonable to infer that the sooner the specific inflammation is subdued the better, and that by so doing the chances of such occurrences are lessened."

For many years, both in hospital and private practice, Mr. Childs has acted on this principle, and his experience at the Metropolitan Free Hospital has afforded him ample opportunity of estimating the relative value of the two treatments. With some few exceptions, he has trusted *entirely* to injections, and has rarely known them to fail. Chloride of zinc, perchloride of iron, and the liquor hydrargyri pernitratis have been employed with the best results. Of this latter Mr. Childs remarks he can speak with the most unqualified praise. For the last six months he has employed it with the happiest results. In some instances six injections have been sufficient to effect a cure, whilst in other cases the treatment has rarely exceeded ten days. In no case has copaiba or cubebs been administered; and, with the exception of a saline aperient at first, reliance has been placed solely on the liquor hydrargyri pernitratis. It would be easy to support this by corroborative testimony in the shape of cases from notes made at the time, but the character of sameness about them would destroy their interest. Suffice it to say that notes have been made, and the results are so satisfactory that Mr. Childs has been induced to draw the attention of the profession to this valuable agent in the treatment of ordinary cases of gonorrhœa. It may be well to state that the strength of the injection employed is half a minim of the solution of pernitrate of mercury to an ounce of water; to be used three times a-day.—*Lancet, Oct.* 12, 1861, *p.* 350.

80.—*Subnitrate of Bismuth as an Injection in Acute Gonorrhœa.*—Dr. Mouslon, a French Military Surgeon, as the result of numerous trials, confirms the good account given by M. Caby of the efficacy of this treatment. He mixes twenty parts of well washed bismuth—if not well washed the acid which remains may give rise to irritation—in 200 parts of distilled water, causing as much to be taken up as possible. Some of this is thrown into and retained for ten minutes in the urethra, a local emollient bath being first employed. In only the severest cases is the patient obliged to maintain absolute rest for four or five days, after which he is enabled to return to his ordinary habits and diet. The cure, indeed, takes place more quickly under the use of this agent than that of any other. In chronic gonorrhœa success is less marked, and astringent injections may afterwards be required; while in confirmed and obstinate gleet, occurring in a broken constitution, the bismuth is of no use. M. Mouslon appends to his communication an account of a mode of treating painful chordee

which he has found very successful. It consists in desiring the patient to sleep on the belly, having first fixed the penis by means of a piece of linen in the fold of the left or right groin.—*Med. Times and Gazette, June* 8, 1861, *p.* 607.

81.—*Gonorrhœa.* By Dr. T. K. Chambers, St. Mary's Hospital.—Gonorrhœa is naturally, in both male and female, a most mild disease, with power to get well in about a fortnight by the simplest treatment, if only it is not made severe by the folly of the patient or his medical attendant. I consider all primary heroic treatment of urethral discharges a most unjustifiable interference with nature.—*Lancet, June* 15, 1861, *p.* 582.

82.—ON THE CURE OF GONORRHŒA BY BLISTERS.

By H. Chalmers Miles, Esq., Assistant-Surgeon Royal Artillery, Halifax, Nova Scotia.

There is nothing novel (says Mr. Miles) in the principle of the treatment I am about to support, whatever there may be in its method and application. Nine years since, "blistering in gleet" was advocated by a gentleman who, in 1853, wrote with still further experience on the subject; but Mr. Milton appears to have "revived or introduced this practice" more with a view to the treatment of *refractory* cases of gonorrhœa or gleet than to recent and ordinary examples of the complaint, for he says, writing in 1851, "Not merely will a single blister frequently cure the most prolonged gleet, not merely will it rapidly sweep away all dregs of the disease in its ordinary course, but it will often cure those runnings which have resisted all known and used methods." And again, in 1853 ("On the Cure of Gleet and Obstinate Gonorrhœa"), "If only pure gleet, sometimes mucous, sometimes purulent, be met with, it will in almost every case yield to a blister." And in 1854 ("On Inveterate Gonorrhœa and Gleet") he says, "My experience in these cases was, that not one in fifty would be cured by ordinary remedies; extraordinary remedies—those called for solely in inveterate cases—were blisters, &c." Whilst in 1855 he adds, "I now, on the slightest sign of persistence (of gleet), proceed to use blisters."

On my taking charge of the batteries of Artillery in this country, I determined to give the plan as ample a trial as my practice permitted, and it is because of the conviction which I entertain that the treatment by blisters is, far above all other, the best suited to soldiers—is the speediest in its result—the most effectual in its cure, and is that which is least open to tampering and deceit, that I now venture to submit some observations on the manner in which it is applied—on the cases to which it is adapted, and on the period within which a complete cure may be anticipated.

[It must be borne in mind that the great proportion of cases of gonorrhœa which are presented for treatment amongst soldiers are of recent origin, as the men are obliged to go to the surgeon as soon as the disease appears. Supposing the running has been noticed about four days, the plan of treatment usually pursued by Mr. Miles is the following:—]

A blister, six inches by four, to be placed very high up towards the anterior and inner aspect of each thigh, is ordered. The blisters are made on adhesive plaster of the ordinary blister of the London Pharmacopœia, the plaster being spread rather thickly, and the surface afterwards sprinkled with acetate of cantharides, a wide margin being left on each side of the blister. They are safely confined in their place by a broad strip of adhesive plaster at each end. For convenience the blisters are generally directed to be applied at night, and no harm has ever come of it, and usually they are well risen by the morning. The patient is then ordered to take every four hours an ounce of the following mixture:—Sulphate of magnesia, two ounces; carbonate of magnesia, four drachms; potassio-tartrate of antimony, two grains; tincture of hyoscyamus, two drachms; peppermint water to sixteen ounces. He is placed on spoon diet, with rice pudding for dinner, and a pint of imperial drink should he be thirsty. During the day he is directed to inject now and then a syringeful of cold or lukewarm water, according to the temperature and season of the year. The blistered surfaces to be dressed with lint dipped in castor oil (a favourite dressing with the men).

The next point for notice is the *immediate* or proximate effect of the blisters on the discharge. On this head you may hear opposite statements, as in some instances the discharge is increased in quantity, and there is more scalding in passing urine, and in others the discharge is rather thicker, but diminished in quantity. Occasionally it happens (see a case narrated below) that the discharge disappears altogether. The most usual result of the application of blisters is rather to aggravate the symptoms for the ensuing twenty-four hours, the discharge becoming more copious, and the pain in micturition being slightly augmented.

The only thing required, however, is a little patience combined with perfect rest, and these symptoms rapidly subside. The saline purgative is to be continued, and the patient is ordered half an ounce of gum arabic to be mixed with water, and used as a drink whenever required, the imperial drink being continued if asked for.

On the third morning after the application of the blisters, the discharge is noticed to be much diminished in quantity, the scalding in micturition is less, and the patient feels better. On the fourth day scarcely any discharge, and there is now no scalding. The blistered surfaces look raw, but are beginning to "skin," as the men say. The saline purgative to be now given only three times a day. The next

day perhaps there is a little running when the penis is squeezed, but no other symptom. His bowels are well open. To take the saline purgative (two ounces) every morning, and an injection of nitrate of silver (six grains to the ounce) to be used at night. Sixth day: Blistered places "skinned," though not firmly, and the surfaces are not sore. No sign of running is visible to-day, but there is an appearance as if of weeping at the lips of the urethra. Seventh day: No sign of discharge; the thighs have completely cicatrized, and the surface where they were blistered is not the least tender. The man is discharged for duty, with one day's convalescent leave. This, though a supposititious case, illustrates the sort of treatment adopted.

But a more favourable case than the above will happen. A lad may come the morning after the discharge appears, and in the early stage of the complaint very mild inflammatory symptoms are present. He is of temperate habits. In such a case all that may be required are, rest and low diet, the saline purgative mixture. and as a local remedy a small blister applied to the under surface of the penis. The blister should be about an inch wide, and from an inch to an inch and a half long, and be carefully fastened on by oiled threads. It is well to fasten a suspensory bandage with some extra linen covering over the scrotum, so as to prevent the chance of vesicating it. If the blister rises properly, in a simple case of gonorrhœa taken thus early, I have had repeated instances of immediate cure by a single application, the discharge having completely ceased, with no recurrence at a subsequent period. In such a case the blistered surface has healed (it is surprising the rapidity with which it does so on this spot), and the man has been discharged for duty on the fourth morning after admission.

In the small proportion of cases in which a relapse occurs (it is generally due to the fact of soldiers who are discharged from the hospital being permitted to go "on pass" in the town, where of course they commit a debauch, and soon require re-admission) I have generally found it yield to the second application of blisters, combined, it may be, with the use of an injection or two of nitrate of silver, and aided by frequent doses of the saline purgative, which is to be given in every instance. It has rarely happened to me to find the first application of blisters inefficient; but when this has been the case, should the thighs have been blistered in the first instance, a blister to the under surface of the penis (of the size already mentioned) has completely checked the discharge; or should the penis have been first blistered, the application of the vesicating plasters to the thighs proved of unfailing efficacy.

Now in illustrating the system of treatment by blistering it will be observed that there are three regions open to choice; 1st, the upper and inner part of the thighs; 2nd, the under surface of the penis; 3rd, the perineum. I have, however, avoided reference to the latter region, as I consider the locality inadmissible in many soldiers by reason of the following objections. If the man's duties require him

to be mounted. I think greater tenderness is left after blistering in this region than in the others. Should small boils appear after the application of blisters, they are unusually troublesome; and it is in this part that the "fork" of the trowsers, if badly made, is apt to cut. In addition to these reasons, as the perineum is always subjected to considerable pressure in mounted duties, should tenderness exist, the man is rendered "ineffective" for parade, and possibly remains so for a considerable time. It will be evident, therefore, that counter-irritation in the shape of blisters applied to the perineum is not suitable in such instances.

To none of these drawbacks, however, is either of the other positions open. I know of no objection to placing blisters on the thighs. Some might suppose that the tendency which often exists to the *appearance of a crop of boils* on the seat of the blister would cause annoyance; but the following treatment will prevent this:—Should some inflamed pustules appear about the roots of the hairs on the new surface, they should be pricked at once and gently squeezed, and a soft linseed-meal poultice applied; if the surface is then well rubbed over with castor oil, there will seldom be any further trouble in the matter. *Tenderness of the skin* on the new surface is removed by painting the part well over with a wash of caustic solution (ten grains to the ounce), by means of a camel's-hair brush. This application hardens the surface, and appears to prevent the cuticle from cracking, &c., on the man's returning to work and out-door occupation.

Is the cure by blisters generally effectual?—According to such experience as I have had I would answer that it is both more speedy and effectual than any I know of.

To what cases is it suited?—Nearly all the cases presented amongst soldiers are examples of recent infection, and the only complication noted is "swelled testicle," in which instance the treatment is just as suitable as in any other. While in the few cases of long-standing gonorrhœa treated, the complaint has yielded to blisters with great readiness.

What accessory treatment is required?—Saline antimonial purgatives at frequent intervals, rest, and low diet, and perhaps—as the blistered surfaces are healing—an injection or two (rarely more) of caustic wash (six grains to the ounce).

What is the period required for cure?—In the event of it being thought necessary to blister the thighs, the soldier is usually returned on the "effective list" on the seventh day from the date of his admission. If the surface of the penis is blistered, and a second application be not required, the man is discharged for duty on the fourth day. Should the case be unusually obstinate, requiring blisters to both thighs and penis, and a recurrence of the application in one or both instances, perhaps he may be in hospital for fourteen days; but this would be rare.

Have relapses often occurred?—Seldom indeed; and then only

after a debauch, or the committal of some imprudence on the man's part.

Has the relapse in such cases been tedious?—In no instance has the re-appearance of the discharge failed to yield to blisters.

What objections can be taken to the plan of treatment?—I know of none in the case of either soldiers or sailors, or other persons employed in the departments attached to the services. The cure I believe to be speedy and effectual. So far as Government is concerned it is economic; and, as regards the patient, it is less painful and annoying than might be anticipated. It cannot surely be very repugnant to the patient's feelings, as several instances have occurred of men employed in the civil departments, and entitled to military medical attendance, coming to me, on getting "clap," with the request to be treated by blisters: "I would rather have *them* blisters, sir, and be cured at once."

Look at the tons of copaiba which must be swallowed by the million! Mr. Weeden Cooke says "that he found upon inquiry at the London Custom-House that 118,396 lb. were admitted into the port of London during the first ten months of 1859." We have here, says he, "copaiba enough to treat 473,584, or close upon half a million persons!" Again, this author asks, "Is it not extraordinary that this disgusting medicine continues so long to hold its ground?" And let us reflect on the many other objectionable medicines which nauseate the seething mass of the population afflicted with gonorrhœa and such like complaints. How far the treatment which I advocate may be applicable to private practice I leave others to judge. Such a question is foreign to my argument, which is limited to cases in the public service.

I beg to add a brief abstract of the foregoing remarks:—

1. That an ordinary gonorrhœa of recent date may generally be cured by the application of a single blister to the penis, in a period of less than four days. Conjointly with the blister, saline purgatives with small doses of the potassio-tartrate of antimony *(vide supra)* should be given at frequent intervals, and rest and low diet rigidly enforced.

2. That in acute gonorrhœa attended with considerable constitutional irritation, occurring in an unfavourable subject, blisters to the thighs, with the concomitants just mentioned, will rarely fail of success; and should the discharge not be checked entirely on the third day after their application, a few injections of solution of nitrate of silver (six grains to the ounce) will usually complete the cure by the seventh day.

3. That in cases of gonorrhœa of several weeks' standing, in which other remedies have been used and without avail, the application of blisters is attended with immediate success.

4. That old obstinate cases of gleet and gonorrhœa occurring in intemperate subjects, and which have resisted ordinary remedies, sel-

dom fail in being completely relieved by blisters; and in instances where the first application is insufficient, a second set of blisters will prove completely effectual.

5. That relapses are infrequent; but that on their occurrence the application of blisters has in no instance failed to effect a cure.

6. That with the exception of saline purgative medicine and the occasional injection of the caustic wash—should slight running continue after blisters have been applied—no other treatment is needed.

7. That the complication of "swelled testicle" in no degree invalidates the success of the treatment by blisters.

8. That no unpleasant symptoms have been noticed after the application of blisters. A crop of small boils occasionally appears around the roots of hairs on the blistered surface; but this is generally no source of annoyance if treated in the manner mentioned above; and and should tenderness remain on the "new skin," the surface will rapidly become "tough" if painted with a solution of caustic (ten grains to the ounce).

9. That when the above circumstances are duly considered—the brief time required for treatment; the small annoyance caused to the patient; and the entire freedom from the administration of all nauseous and offensive medicines; with the completeness of the cure generally effected,—it is thought that the treatment of gonorrhœa by blisters will be acknowledged to offer the means best adapted for use in the instance of men in our armies and fleets.—*Lancet, June* 8 *and* 22, 1861, *pp.* 558, 605.

83.—*Chlorate of Potash in Gonorrhœa.* Dr. IRVIN, United States Army, stationed at Fort Buchanan, says—

"It may not be amiss to place on record my testimony in favour of the use of chlorate of potash as a therapeutic agent, which I have constantly used during the last two years in the treatment of gonorrhœa, both in my public and private practice, with the most gratifying results. I have found it to be such an admirable remedy that I seldom resort to any other in the treatment of urethral inflammation. My method of using it is as follows:—One drachm of the salt dissolved in eight ounces of water, of which an injection is given every hour for twelve hours, at the end of which the discharge has become changed and diminished, allowing the remedy to be gradually discontinued until the second or third day, when the disease will be generally found to have ceased. So efficacious has this remedy proved in my practice, that I seldom deem it necessary to give any other medicine, save a Seidlitz draught or a dose of Epsom salts."—*Medical and Surgical Reporter, and Glasgow Medical Journal, July* 1861, *p.* 255.

84.—HOW IS SYPHILITIC TO BE DISTINGUISHED FROM NON-SYPHILITIC PSORIASIS.

By Dr. T. M'Call Anderson, Physician to the Dispensary for Skin Diseases, Glasgow.

Diagnosis of Syphilitic from Non-Syphilitic Psoriasis.

SYPHILITIC PSORIASIS.	NON-SYPHILITIC PSORIASIS.
1. Eruption *not usually* very extensive.	1. Eruption *often* very extensive.
2. Patches usually very small, and in shape of spots (size of a split pea), or of *small* circles or segments of circles (seldom more than an inch in diameter).	2. Patches *often* very large and irregular. When circular, circles often two or three inches in diameter.
3. Eruption not usually on the elbows and knees; more on the inner than the outer aspect of limbs. When limited to soles or palms, usually syphilitic.	3. Eruption on any part of the body, but *almost* invariably on the *elbows and knees also.*
4. Eruption usually of a distinctly coppery tint, after it has become chronic; sometimes very dark, even nearly black *(psoriasis nigricans).*	4. Patches of a dusky-red or light coppery colour.
5. Scales thin; not so imbricated; often greyish.	5. Scales thick, imbricated, white, and silvery.
6. May last months, or even one or two years, when no treatment employed.	6. Often of five, ten, fifteen, twenty, or thirty years' duration, or even almost a whole lifetime.
7. Almost always commences after puberty, and usually after 20.	7. Most cases (not all) commence before the age of 20.
8. Rarely if ever itchy.	8. Sometimes not itchy; sometimes intolerably so; generally *slightly* itchy *now and then.*
9. A relapse not usual after *all trace* of the first eruption has *completely* disappeared.	9. Relapses are the rule, and are often very numerous.
10. Can often be traced to a hard chancre.	10. Can often be traced back to hereditary transmission.
11. Patient *may* be cachectic, and concomitant symptoms detected; *e.g.* roseola syphilitica, lichen syphiliticus, condylomata, sore throat, alopecia, &c.	11. Patient in robust general health; bad health incompatible with the eruption, and the eruption is antagonistic to scrofula.
12. Removed almost invariably by mercury.	12. In the majority of cases, removed partially or entirely by arsenic.

[Dr. Anderson gives the two following cases illustrating the effects of remedies in these forms of disease.]

The Effects of Remedies.—It must be known to all that mercury is a never-failing remedy for the removal of syphilitic psoriasis, an example of which is furnished by the case of Mrs. Davidson, aged 39, who consulted me on April 3, 1861, (the minutiæ of the case I am unable to give, as I write from memory, not having taken notes of it at the time), with syphilitic psoriasis in the shape of very distinct little coppery circles, covered by white scales. None of these circles were more than an inch in diameter, and were situated principally on the extremities. After twelve calomel vapour baths, no trace of the eruption was left, except in the shape of very faint red circles—all the scales, the coppery tint, and the elevation above the surface having disappeared. She was told to continue the baths, and as she never returned there can be little doubt that she was completely cured. The vapour bath was extemporized according the suggestion of Mr. Henry Lee. She was told to heat to redness half of a common brick, to place this in a common pan, the bottom of which contained a little boiling water. She was then to place the calomel powder in a thin layer (20 grains) upon the surface of the brick which was not in the water, to put the pan beneath a cane-bottomed chair, and to sit on the chair, enveloped in a blanket, which was drawn *tight* about the neck—to prevent the mercurial vapour getting into the mouth, causing sickness and salivation—but covering the body *loosely*, so as to allow the vapour to have free access to and to act upon the skin. The hot brick has the effect of keeping the water boiling, at the same time as it volatilizes the calomel.

Arsenic, on the other hand, is the remedy *par excellence* in the treatment of the non-specific form of the disease, and to which a large proportion of the cases yield. As an illustration of this may be taken the case of William Martin, which occurred at the Dispensary for Skin Diseases, under the care of Dr. Buchanan. This patient, a blacksmith, aged 25, was admitted March 5, 1861, with psoriasis covering almost the whole body, though leaving intervals of sound skin between the patches. It was the most extensive eruption of the disease I ever saw, and had existed more or less for five months. The joints were uneasy and painful, owing to the deep fissures which existed, and the hands were quite covered with the eruption, and so stiff and painful that the patient was unable to open the dispensary door on the day of his admission. He was ordered five drops of Fowler's solution thrice daily, and the dose was gradually increased up to fifteen drops. The exhibition of this dose soon occasioned slight pain in the epigastrium, and the conjunctivæ became suffused, so that it was diminished to six drops three times daily. On the 19th April there was merely redness of the skin left in the sites of the previous patches, and on the 26th almost all trace of the disease had disappeared.—*Glasgow Medical Journal, July* 1861, *p.* 160.

DISEASES OF THE EYE AND EAR.

85.—ON THE RELIEF OF SYMBLEPHARON BY TRANSPLANTATION OF CONJUNCTIVA.

By T. Pridgin Teale, Jun. Esq., M.A. Oxon., &c.

[Mr. Pridgin Teale gives us the following mode of operating in cases where adhesions of the eyelids to some contiguous parts interfere considerably with vision. It has generally satisfied surgeons to divide the bands or adhesions, as they would the cicatrices from burns, and keep the parts asunder as much as possible, till healed; but the subsequent cicatrix often makes the case as bad as before. Mr. Teale, therefore, very cleverly adopts the same principle with respect to the eyelid as is applied to cicatrices of the skin, in the following way:]

Mode of operating.—Having first made an incision through the adherent lid, in a line corresponding to the *margin* of the concealed cornea, I dissected the lid from the eye-ball until the globe moved as freely as if there had been no unnatural adhesions. Thus the apex of the symblepharon being part of the skin of the lid, was left adherent to the cornea.

In the next place two flaps of conjunctiva were formed, one from the surface of the globe near the inner extremity of the raw surface, the other from the surface of the globe near its outer extremity. I first marked out with a Beer's knife a flap of conjunctiva, nearly a quarter of an inch in breadth, and two-thirds of an inch in length, with its base at the sound conjunctiva, bounding the inner extremity of the exposed raw surface, and its apex passing towards the upper surface of the eye-ball. The flap was then carefully dissected from the globe until it was so far at liberty as to stretch across the chasm without great tension, care being taken to leave a sufficient thickness of tissue near its base. A second flap was then made on the outside of the eye-ball in the same manner. In making the flaps, conjunctiva alone was taken, the subconjunctival fascia not being included. The two flaps thus made were then adjusted in their new situation. The inner flap was made to stretch across the raw surface of the eyelid, being fixed by its apex to the healthy conjunctiva at the outer edge of the wound. The outer flap was fixed across the raw surface of the eyeball, its apex being stitched to the conjunctiva, near the base of the inner flap. Thus the two flaps were dovetailed into the wound. The flaps having been adjusted in their new position, their vitality was further provided for by incising the conjunctiva near their base, in any direction in which there seemed to be undue tension, and by stitching together the margins of the gap whence the transplanted conjunctiva had been taken. One or two other sutures were inserted, with a view to prevent doubling in of the edges of the transplanted conjunctiva.

[Four cases are then related in which the operation was performed, and from which Mr. Teale draws the following conclusions:]

1. That conjunctiva may be transplanted without losing its vitality or properties.

2. That a symblepharon may be replaced by loose moveable conjunctiva, at least equal in breadth to the flaps originally transplanted.

3. That a comparatively small breadth of conjunctiva introduced into the situation of a symblepharon is sufficient to afford greatly increased, if not perfect, freedom of motion to the eye-ball, a freedom which continues to increase for many months after the operation.

4. That where the conjunctiva of the lateral and upper parts of the eyeball has not been damaged, flaps of a quarter of an inch in breadth may be taken away without giving rise to any deterioration of the parts whence they have been taken.

5. That in separating the adherent eyelid from the globe, it is not necessary to dissect off from the cornea any portion of skin that may be adherent to it, but that is better to commence the separation of the lid at the margin of the cornea, leaving the opaque apex of the symblepharon adherent to the cornea.

My object in commencing the separation of the lid at the margin of the cornea, thus leaving an opaque island of skin on the cornea, was two-fold. In the first place, it seemed more than probable that, if the corneal surface had been denuded of its adherent skin, any conjunctiva transplanted into the gap would be drawn by cicatrization over the newly-made raw surface of cornea, as the skin of the eyelid had been in the first instance. The conjunctival flap would thus have been rendered useless, and the opaque skin would have been replaced by not very transparent conjunctiva. In the second place, I had a faint hope that the isolated portion of skin, being cut off from all continuity with skin, and depending for its nutrition on the cornea supporting it, and a merely *cicatricial* union with conjunctiva, might become atrophied and less opaque. The result has far exceeded my expectations. In the case of Jessop, the trace of this skin can only be detected in a good light. In two other cases the situation of former frœna can only be seen on minute inspection. In the case of Spence, operated on last March, the edges of the opacity have become transparent, and the remaining portion is atrophied, and but little raised above the level of the surrounding cornea. —*Ophthalmic Hospital Reports, Oct.* 1861, *p.* 254.

86.—*Presbyopia.*—By Mr. J. Soelberg Wells.—A very rapid increase of presbyopia is one of the premonitory symptoms of glaucoma. If, therefore, a patient tells us that his far-sightedness has rapidly increased within a few months, so that he has had repeatedly to change his spectacles during that time for stronger and stronger ones, our suspicions should be aroused, and we should without fail examine him

as to the presence of other premonitory symptoms of glaucoma—*e.g.*, rainbows around the candle, periodical obscurations, &c. Von Graefe thinks that this rapid increase of presbyopia is most likely due to an increase of intraocular pressure and flattening of the cornea.—*Med. Times and Gazette, June* 1, 1861, *p.* 575.

87.—*Black Eye.*—A writer in the 'Medical Times and Gazette' says:—"A correspondent" asks the communication of an effective remedy for ecchymosis of the eye in a lady patient who had met with an accident. One which I have used for a great many years, never disappointing me, usually getting rid of the blackness in a few hours, is the root of the "Convallaria," "Solomon's Seal." It should be washed, the dark-coloured cuticle carefully cut off, then scraped like horse-radish, applied direct to the eye in the way of a poultice, cold. A tingling sensation is the consequence; when this sensation ceases another fresh application should be made, and repeated until the whole ecchymosis is absorbed. I have often found it sufficient to apply the scraped root at bedtime to the closed eye, when the blackness has disappeared by the morning. The convallaria is readily found, and is equally effectual at this season of the year.—*Dublin Hospital Gazette, June* 1, 1861, *p.* 175.

88.—ON CATHETERISM OF THE EUSTACHIAN TUBE, AND ON THE PHARYNGOSCOPE.

By Dr. Voltolini, District Physician in Breslau.

Dr. Semeleder, of Vienna, has published in the Austrian Journal of Practical Medicine an interesting essay on the above subject; but he therein attributes to special aurists a peculiar dexterity in catheterising, so that it might be supposed that the employment of the pharyngoscope, at least for the purpose of controlling the situation of the catheter, might not be so necessary to such practitioners. It will therefore not be superfluous for me, as a practical aurist, to write something on the catheterism of the Eustachian tube, especially as I do not only daily practise this operation, but also almost daily employ the pharyngoscope; indeed, I think it even necessary to write something upon catheterism, because, in consequence of the introduction of the pharyngoscope, that operation, and with it aural surgery, have assumed a new phase. Pharyngoscopy first reveals to us all the mistakes hitherto made, and still daily made in catheterising, and of which, to a certain extent, the operator could have no suspicion. Even our anatomical knowledge of that region of the body is enlarged, as we can now every moment by inspection study the parts in the living state, whereas we cannot every moment have a dead body to examine. The previous want of anatomical knowledge is evidenced in the description of catheterism. Thus, for example, the beginning of the description of this operation by Kramer is incorrect, although

undoubtedly Kramer contributed largely to the improvement of the operation. He says: "If the catheter is introduced into the nose so far that its point touches the posterior wall of the pharynx (whereby the outer visible ring, consequently also the concavity of the beak, is still directed downwards) we raise the posterior end of the catheter, the beak sinks and glides, while we carefully draw the catheter to ourselves, over the posterior roundish projection of the mouth of the Eustachian tube, &c." Now this is incorrect: if I guide the catheter, with the beak directed downwards, straight through the nose to the posterior wall of the pharynx, and then draw it back in the same direction, I cannot pass the projection of the opening of the tube, and it is quite indifferent whether I depress or raise the beak of the catheter; if the catheter remains in the same direction, that is, with the beak downwards, it passes freely through the naso-pharyngeal cavity to the posterior wall of the pharynx, and as freely again back to the velum palati. But if the catheter in this course gets into a depression, as perhaps into the opening of the tube, we have to do with a pathological condition. As I write this, I repeat the experiment on myself, and with the pharyngoscope upon a patient; I now feel and see the catheter passing quite freely through the naso-pharyngeal cavity; it is not until the beak of the instrument is turned outwards, that it touches the orifice of the Eustachian tube—and sometimes not even then, if a catheter with a long beak be not employed. This relation we could certainly without the pharyngoscope demonstrate in the dead body, but there are other circumstances respecting which we could not in every case without this instrument obtain information. Incidentally to the term "pharyngoscopy," I may observe that I shall enter more fully into this subject in a paper which is about to appear in Virchow's *Archiv;* here I shall confine myself to it so far as it relates to aural surgery, having nothing to do with the inspection of the nose (Rhinoscopy).

As to the catheterism itself, I speak only of the method through the inferior nasal passage—other methods depend in part on incorrect anatomical views, or are quite unsuitable and much more complicated than this. In catheterising through the middle nasal passage, we arrive over the mouth of the tube, as unfortunately more frequently happens in attempting the operation through the inferior passage; but neither does it then succeed, and the catheter must be again taken out, and introduced into the inferior passage. I have not yet had occasion to catheterise through the mouth, and this method would be applicable only to those cases where the nose is rendered impermeable by abnormal formations.

In catheterising through the inferior nasal passage, there is only one fixed rule, namely, to keep with the catheter on the floor of the nasal cavity; all other rules are liable to numerous exceptions. In general the operator passes through the nasal passage with the beak of the catheter directed downwards, and towards the septum. But

cases occur where it must be directed upwards both in entering and passing through the nose; an instance of which is at present under my care. In general, if any resistance is met with, the catheter is made to rotate gently between the fingers, and, so to speak, to find the way for itself—only no force should be employed; this is the first immutable condition, just as in catheterism of the urinary bladder (unless one has a special passion for forcible catheterism). In like manner, there are cases where the floor of the nasal cavity passes obliquely outwards and downwards, and is, as it were, hollowed out; in this case the catheter must be kept far from the septum. If in difficult cases we cannot, by twisting and turning the catheter in various directions, pass through the nose, a very slight catheter must be taken, and after having ascertained with it how the obstruction is to be avoided, we must then try a stronger one.

Having passed the catheter through the nose, the rule must be now to turn the beak of the instrument downwards, if it has not already in its transit been held in this direction; this usually easily succeeds, because the operator has sufficient room in the naso-pharyngeal cavity, that is, if the beak be first in the cavity turned to the other nostril, and then turned downwards; generally all this is not necessary, but the catheter is passed immediately with the beak directed downwards, straight through the nose to the posterior wall of the pharynx. If the latter has in either mode been reached, two ways are open of bringing the catheter into the mouth of the Eustachian tube. Either its beak is turned immediately outwards on the posterior wall of the pharynx, whereby the instrument is brought into Rosenmüller's cavity, therefore behind the projection of the tube, and is now drawn gently, feeling our way, straight forwards over the prominence of the orifice: and so soon as it is felt that it has glided over the latter, it is gently pressed into the mouth of the tube; or the beak of the catheter is drawn, directed downwards, from the posterior wall of the pharynx directly forwards back to the velum palati (in so doing it is well to press the beak with somewhat greater force still more downwards, in order to be sure of hitting on the velum, and not by more gentle manipulation to run the risk of returning into the nose); having arrived there, the beak is turned outwards and somewhat upwards, and pushed *a little* forwards, whereby it passes into the mouth of the Eustachian tube. The latter operation is often still further facilitated by causing the patient to make the movement of swallowing in the moment when the quarter-turn is made, and the catheter is thus usually involuntarily thrown into the mouth of the tube.

What I have hitherto said respecting catheterism is calculated to be of use only to those who are little practised in the operation; for the aurist in full practice, these are matters of daily experience. But I shall now state some circumstances first discovered by the pharyngoscope, and which are of importance also to the aurist. Thus, if we inquire what proof we have that the catheter is actually in the

tube, the one aurist will say, "I feel the catheter gliding over the prominence of the tube;" the other will answer, "I hear the air, when I blow into the catheter, penetrating through the tube into the cavity of the tympanum." It would be all right, if I feel that and hear this; but how if I neither feel that nor hear this? then I have no certain guide but the pharyngoscope; and these cases are not so rare. For example, if we have to do with a case of obstruction of the tube, we cannot hear the air penetrate, and if the projection of the tube be at the same time not perceptible, in consequence of its being too small or lying too much externally, this ground of support also fails. But now there occurs, even frequently, a still more remarkable case, namely, on blowing through the catheter, we hear the air very distinctly penetrating through the tube into the cavity of the tympanum, and still the catheter is not in the tube. In such instances we see, through the pharyngoscope, the beak of the catheter not in the mouth of the tube; but at a greater or less distance from it, in a direct line inwards, the mouth of the catheter looks, as it were, from afar into the orifice of the tube. If I now blow strongly into the catheter, the current of air passes of course directly out into the mouth of the tube, through this into the cavity of the tympanum; and on auscultation, I shall hear the air as well as if the catheter were actually in the tube. I may thus attain even great therapeutic results by removing mucus, &c., and so making the tube permeable, I may be still further confirmed in my opinion that the catheter is really in the tube. But the matter is quite different, if in such a case I attempt to introduce vapours or a catgut into the tympanum. The vapours then extend around the mouth of the tube without reaching the cavity of the tympanum, or at most they arrive there in very small quantity, because they cannot be constantly driven in with such force as a powerful lung exerts in driving the air in jerks through the catheter. Matters are still more unfavourable to the introduction of a catgut: if the catheter be not well fixed in the tube, the catgut in general bends when it comes out of the mouth of the catheter, and passes down into the pharynx—I shall not venture to say where a probe or pointed instrument introduced through the catheter would reach to. But in those cases where neither the prominence of the mouth of the tube is felt, nor the air is heard to press through the tube, no particular mischief will be done if merely air or a drop of medicine be blown in—they will simply not penetrate the tube. But if I suppose that the catheter is actually in the tube, because I have perhaps made the regular turns with it, and if I wish forcibly to remove the supposed obstruction of the tube, I might cause great mischief, nay, even the death of the patient. It is well known that the dreadful misfortune happened to a practitioner in London, that not one, but two patients fell dead, in consequence of the incautious use of the compression pump, so that he was of course brought into court about it. But the pharyngoscope reveals still other positions of

the catheter; we may more frequently observe that its mouth is placed at one side of the orifice of the tube, that is, it will be either partly within the latter, or may be so placed upon a margin of the prominence as to look partly, perhaps half, into the orifice; in these cases, too, on strong insufflation, the air is heard to pass through the tube, while for other operations the above described obstructions exist.

Dr. von Troeltsch, in his valuable work on the anatomy of the ear has shown, that in children the pharyngeal mouth of the tube projects less into the pharynx; and that the slender lips of the simply split-like opening usually lie so near one another, that in the dead body it is often difficult to discover them in the swollen mucous membrane of the throat. This is a very important circumstance; for I have repeatedly in patients of from twelve to sixteen years of age found the parts still in the condition they present in infancy (and probably the same state sometimes occurs also at a later period of life), that is, an arrest of development, where the mouth of the tube still presented that lip-like split. In such cases it is a chance if the catheter will ever be brought into the tube, for the soft lip-like fissure cannot be felt. In this instance it is not enough to inspect the parts with the pharyngoscope, but the catheter must be introduced with the assistance of the latter, that is, it must be introduced during examination with the pharyngoscope. In this I am already so practised, that with one hand I can manipulate my spatula-speculum, while with the other I introduce the catheter. In another paper I shall shortly describe my mode of proceeding, and also communicate some cases where this arrest of development of the tubular opening occurred, and which were successfully operated on. Meantime I shall here mention only that raising the uvula in pharyngoscopy is a point I have long since got over, that is, I no longer trouble myself about raising the uvula in any way; but with my little spatula-specula, as prepared according to my directions by Hauck, of Vienna, I almost daily inspect the tube, without at all displacing the uvula. The new venturesome instruments recently brought out for raising the uvula are not only unpractical, but superfluous. It is not the uvula, but the contraction of the soft palate, which renders pharyngoscopy or rhinoscopy difficult; therefore it is of no use to raise the uvula, all above remains as dark as before; and, on the other hand, if the contraction of the palate has been overcome, if the pharyngo-nasal cavity be free, it is again not necessary to raise the uvula, the speculum can be very well used without that. There are very rare cases in which, even when the soft palate is relaxed, it is necessary to raise the uvula, in order to see very freely into the cavity. This is the case where the uvula is very broad and long, so that we cannot even see through the two arches of the palate, or if we should wish to look very deeply into the posterior nares. There are noses where, if the clear sunlight be allowed to fall into them, a very thick catheter having been at the same time introduced, we can see directly through them to the poste-

rior nasal wall, and follow with the eye the movements of the catheter in the naso-pharyngeal cavity. In such instances, therefore, no speculum nor instrument for raising the uvula is necessary to enable us to inspect the nose.

There is still another mode of catheterising, in which the pharyngoscope is of essential use, that, namely, where from one nostril we pass the instrument into the tube of the opposite side, if the corresponding nasal cavity is in any way malformed. In such a case we make use of a catheter with a longer beak. Dr. Erhard, of Berlin, doubts the practicability of this operation, and Kramer describes it imperfectly. He says when the posterior wall of the pharynx has been touched by the point of the catheter, turning the latter on its axis towards the affected ear will, on account of the longer beak, in general succeed only with greater difficulty, and best at the moment when the patient draws in his breath, or makes the motion of swallowing. Now this is, properly speaking, no description at all; for it would be a mere accident, if in such manipulation the catheter entered the tube. I have already, in March, 1860, in the hospital here, and on the second of July in the Association of Medical Science of Berlin, exhibited this operation on myself; the gentlemen who were present at these demonstrations saw very plainly, in the pharyngoscope, the catheter pass from the one nostril into the opposite tube. The operation must be so performed, that the beak of the catheter may pass directly into Rosenmüller's cavity of the opposite side, that is, in the diagonal of the naso-pharyngeal cavity; then, in drawing back the catheter, it must be guided gently over the posterior projection of the tube, and so into the latter, as in ordinary catheterism.

I believe that I have in this paper shown what the defects of the art of catheterism have hitherto been, and that the pharyngoscope can no longer be dispensed with in aural surgery. Pharyngoscopy and rhinoscopy are certainly more difficult than laryngoscopy; they will, however, undoubtedly be more cultivated, and the instruments will be improved. Semeleder's illuminating spectacles are an excellent instrument for all investigations of this kind, particularly for near-sighted people, who can have glasses adapted to them inserted in the frame; these spectacles do not incommode the operator in speaking, as the specula with a mouth-stalk do; which is not unimportant, as it will often be necessary during the operation to give directions to the patient how to hold himself. I regret not having sooner possessed them.

In conclusion, I may mention an anatomical peculiarity of the nose, which I met with in the greater number of skulls in the museum here. The nasal septum is mostly bent to the left side, and the left nostril is consequently narrowed; this observation first explained to me why in most men catheterism is more difficult on the left side than on the right, and why we can scarcely ever introduce so thick a catheter on the left side as on the right.—*Medizinische Jahrbücher; and Dublin Quarterly Journal, Aug.* 1861, *p.* 223.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

89.—ON THE INDUCTION OF PREMATURE LABOUR.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

Professor Simpson said that he had brought on labour prematurely, about four years ago, in a case where he believed that he had met with a new indication for the performance of the operation. Mrs. L., the patient who was the subject of the operation, had fallen in labour of her first child in 1851. Her labour had lasted two days, and was terminated by the head of the child, which was in a hydrocephalic condition, being perforated by Dr. Jamieson of Peterhead. Her second labour, in 1853, was also terminated by means of the same operation, by Dr. Johnston of Stirling, because of the same morbid condition of the fœtal head. In 1856 he (Dr. S.) first saw the patient, in consultation with Dr. Johnston, at a time when she had arrived at the eighth month of her third pregnancy. There was every probability that in this instance again the child would become hydrocephalic, and be destroyed at birth; and the probability was increased by the circumstance that the only sister of the lady in question had given birth to but one child, which had likewise been hydrocephalic, and had been delivered by means of craniotomy. The likelihood of the recurrence of the disease in the third fœtus was still further impressed on his (Dr. Simpson's) mind, by a letter which he received about that time from Dr. Embleton of Embleton, in which that gentleman related how he had just delivered a woman of a child that presented by the breech, and was very difficult of extraction, because of the head being distended to twice the natural size from hydrocephalus. And Dr. Embleton had stated, further, that the first and third children of the same patient had been born hydrocephalic, and had been delivered by means of the forceps. Regarding it, therefore, as in the highest degree probable, in the case of Mrs. L., that if her third pregnancy were allowed to run its full course her third child would become the subject of intra-uterine hydrocephalus like its predecessors, he (Dr. S.) had two reasons to lead him to induce premature labour at once; viz., 1st, it was a matter of great importance for the lady to have a *living* child, and there was little prospect of her hopes being fulfilled if her progeny were all born hydrocephalic; and, 2ndly, it was fairly to be expected, that if the child could be brought into

the world at once, and the conditions of its existence changed, it would have a better chance of escaping the dangers of the disease which had proved fatal to the first two children. The result justified his (Dr. S.'s) expectations. Labour was induced, and a living child was born, which he had seen a few weeks ago in the employment of the most perfect health. The method by which labour was induced in that instance, was one which he (Dr. S.) hoped would never again be employed. At that time, the means most commonly adopted for the purpose was the injection of tepid water into the maternal passages, and the manner in which this process acted in bringing on labour was then a frequent subject of discussion. He had become convinced that its *modus operandi* depended on the separation, which it effected, of the membranes from the interior of the uterus, and which he had come to regard as the exciting cause of parturition at the ordinary termination of a normal pregnancy. As the injection of water was attended with this drawback, that the bed and body-clothes of the patient became disagreeably moistened by the fluid that escaped, he determined to try whether the separation of the membranes might not be as effectually accomplished by some less discomforting means; and, accordingly, he attempted to effect his purpose by the injection of atmospheric air, in the case of a patient whom he had seen before in consultation with Dr. Smith of Lasswade, and in whom he had induced premature labour on a former occasion. He introduced the air by means of a catheter passed into the interior of the uterus, between its walls and its contained membranes, on two successive days; and labour supervened within a few hours after the second inflation. In the case of Mrs. L., however, he had used neither water nor atmospheric air, but carbonic acid gas, which had just at that time been proposed as an oxytoxic by Scanzoni. The gas was introduced for the first time by means of a catheter on a Wednesday, and its injection was repeated once each day till the following Sunday. Labour pains supervened on the Saturday, and continued feeble and intermitting till the Monday evening. The os uteri was firm and cartilaginous, and a number of small incisions were made into it, to favour the dilatation. The child was not born till Tuesday morning, or for six days after the first injection of the gas. The case, however, was so far successful. But he (Dr. S.) believed that a similar operation *ought never again to be attempted.* For, having happened soon afterwards to speak of these cases to some of his confreres in another medical school, one of them, who was induced to adopt the operation, had the misfortune to lose his patient. The gentleman in whose hands the accident happened was a distinguished and experienced practitioner, and performed the operation with every care and precaution; yet the patient died in the course of a few minutes. The fatal effect was probably due to the separation of a portion of the placenta, and the entrance of air through the uterine sinuses into the circulation. The case was not solitary, however; for Scanzoni had recorded two

fatal cases of the injection of carbonic acid gas into the uterus; and in a case in which the artificial and criminal induction of abortion, at Philadelphia, was attempted by blowing in air into the uterus, the patient suddenly expired. He (Dr. S.) therefore maintained that the injection of air or gas of any kind into the interior of the uterus, for the purpose of inducing premature labour, was an operation which we were not entitled to repeat. He (Dr. S.) had many times had recourse to the injection of tepid water for the induction of labour,—sometimes by means of Kiwisch's douche, which was a clumsy and inconvenient contrivance; more frequently by means simply of Higginson's syringe, the nozzle of which could be introduced within the os uteri. But, besides being attended with discomfort from the wetting of the bed, this operative procedure was open to the further objection, that it sometimes brought about a change in the presentation of the child. Moreover, he (Dr. S.) had been greatly alarmed by seeing a patient on one occasion faint under its use, probably from some of the fluid getting into the circulation. And in consultation practice he had seen two more alarming cases still, where both the patients died. In both, only a few ounces of water were injected; and yet rupture of the uterus took place. The occurrence of the rupture was to be explained, as he (Dr. S.) believed, by the fact, that the uterus being already fully distended, could not admit the few ounces of fluid necessary for effecting the separation of the membranes from its interior without being stretched and fissured to some extent; and during labour these slight fissures might easily be converted into dangerous and fatal ruptures. In one of the cases he referred to, the patient died before labour was completed; the other, in twelve hours after its termination. They both occurred in the hands of most accomplished and able accoucheurs; and he (Dr. S.) thought the procedure ought to be entirely set aside, or should be had recourse to only in some exceptional circumstances. Fortunately, we had other and safer means of effecting our object; and of these, perhaps, the best was that now in very common use in Germany, viz., the introduction of a flexible male catheter into the interior of the uterus between the membranes and the uterine walls, withdrawing the stilet and leaving the catheter there until labour set in. The history of that operative procedure was, shortly, this: Dr. Hamilton used to bring on premature labour by separating the membranes from the interior of the cervix, to a distance of two or three inches, with his finger or a catheter. The plan was not very effectual, and required always to be repeated very frequently before labour supervened. About eight years ago, he (Dr. S.) had supposed that it might be produced more effectually by leaving a foreign body in the cavity; and chose as the subject of experiment a patient on whom labour had on two previous occasions been successfully induced by means of the water douche, and who had had several disappointments from habitual death of the fœtus in the two last months of utero-gestation. On the occasion referred to, the

membranes were separated to a distance of two and a half inches from the interior of the cervix by means of an intra-uterine pessary, which was left permanently in the cavity, in the expectation that labour would speedily supervene. It was moved about from time to time, but no signs of approaching labour appeared. The fœtal heart continued to beat vigorously; and as it seemed quite probable that it might be carried to the full time with perfect safety, no supplementary measures were taken to insure the premature occurrence of parturition. The pessary was thus worn for thirty-one days in the cervix, and was frequently moved all round its interior, without the slightest effect; for labour only supervened at length on the completion of the full term of pregnancy. Believing that labour could be more certainly induced by the separation of the membranes from the interior of the *body* of the uterus, he (Dr. S.) had afterwards attempted to do this by passing the uterine sound to some distance into the interior of the uterus, and moving it slightly from side to side, taking care to avoid the site of the placenta. The success attendant on this form of operation was so great, that for some years past he (Dr. S.) had used the sound almost exclusively for inducing premature labour. It had one disadvantage, however, inasmuch as in some cases it was found necessary to introduce it several times before labour was finally brought on; and in one case that had occurred in his practice some months ago, six days elapsed from the time of the first introduction of the second ere labour finally supervened. The retention of the flexible portion of the male catheter in utero overcame this difficulty. If placed between the uterus and membranes, labour seemed almost invariably to supervene within twelve hours. If its extremity chanced to perforate the membranes during its introduction, labour did not supervene in some instances for twenty or thirty hours longer. In Germany they were in the habit of introducing a gum elastic male catheter into the uterus, and leaving it there with the stilet till labour supervened. Fearing lest the uterus might be injured by the presence of a firm foreign body, he (Dr. S.) had withdrawn the stilet, and left only the flexible catheter, which remained until labour supervened. Statistics of fifty cases that had been operated on by this means in Germany had been collected by Dr. Amann, who showed that, in all, labour had been induced within twelve hours. He (Dr. S.) believed, therefore, that it would be found to be an improvement in the operation to leave a foreign body in the cavity of the uterus; but perhaps an india-rubber tube or some soft body would be found to be as safe an instrument, and quite as successful a one, as the elastic catheter. Dr. Simpson added, that in some special cases other modes of treatment might require to be adopted, and stated, in particular, that where it was desirable to have the os uteri dilated to some extent before labour came on, the sponge tent might be employed with advantage.—*Edinburgh Medical Journal, Sept.* 1861, *p.* 289.

90.—INDICATIONS AND OPERATIONS FOR THE INDUCTION OF PREMATURE LABOUR, AND FOR THE ACCELERATION OF LABOUR.

By Dr. Robert Barnes, Physician to the Royal Maternity Charity, London.

[Much attention has been devoted to this subject lately, and its use revived. It was practised many years ago, but had become obsolete and forgotten.]

The only method used for a long time was the puncture of the amniotic sac directly over the os uteri. Experience showing that the premature escape of the liquor amnii led to the frequent peril of the child from undue compression in being forced through an imperfectly dilated cervix, Hamilton, to obviate this objection, substituted the separation of the membranes for a given space from the lower segment of the uterus; and Hopkins tapped the sac at a point remote from the os. Others gave ergot of rye. Merrem and Crause, in Germany, placed a flexible catheter in the uterus, leaving it there to excite pains This plan was now adopted by Braun, of Vienna, who spoke highly of it. In 1820, Bünninghusen led the way in a series of proceedings for dilating the cervix by plugs and instruments of various kinds. In 1842, Hunter proposed to expand the vagina by inflating bladders introduced in a flaccid state. Braun then introduced his caoutchouc dilator. These contrivances have been followed by inflammation of the genitals and death. The application of galvanism, of the irritation of the breasts, of the carbonic acid gas douche, were adverted to. The carbonic acid douche to the vagina had caused sudden death in the hands of Scanzoni. In 1846, Kiwisch introduced the vaginal warm water douche; soon after Cohen, of Hamburg, revived a proposition of Schweighaüser, in 1825, to inject water into the cavity of the uterus. This, the intra-uterine douche, was now extensively used. The action and merits of the douche were discussed. Although easy and convenient, it was not free from objections. Dr. Guillier had related a case in which sudden death followed a vaginal douche, it being conjectured that air had passed with water through the Fallopian tubes into the peritoneum. Other fatal cases are known. The free injection of water into the uterus might detach the placenta; it might stretch the imperfectly developed uterine fibres beyond their power of reaction, causing paralysis. The author then adverted to the recent application of the caoutchouc dilator inside the os and cervix uteri for the purpose of dilating these structures. He quoted the case of Mr. Murray, and referred to the claims of Dr. Keiller and Dr. Storer. He then related two cases of placenta prævia, in which he had himself accelerated labour, successfully opening the cervix for delivery by turning by aid of water-power applied to the interior of the cervix. He also related the histories of three cases in which

labour was induced and accelerated by the same method: one being a case of distortion of the pelvis simply, one of distortion complicated with cicatrised os, and one of almost complete obliteration of the cervix by cicatricial tissue. He described the instruments employed, and the advantages attending their use. The caoutchouc dilator acted gradually and gently; unlike the douche, it admitted of being applied to the exact spot where a dilating and exciting force was required; as a means of overcoming rigidity, it was more certain and efficient than any other, superseding bleeding, incisions, tartar emetic, and the stretching by the hand. As an accelerator of labour, it was so perfect as to supersede altogether the *accouchement forcé*, an operation which might now be exploded. It enabled the Practitioner to deliver almost at will, not only on a fixed day, but at a predetermined hour, a power that gave us control over cases of convulsions, obstinate vomiting, exhaustion from disease or hemorrhage, much needed and not hitherto possessed. In connexion with this subject, Dr. Barnes adverted to the importance of the method described by Dr. Braxton Hicks, of turning without passing more than two fingers into the uterus, by combined internal and external manipulation. Whenever expulsive pains could not be excited, this operation enabled the Practitioner to complete the delivery on the spot. The author concluded by classifying the methods of inducing labour according to their therapeutical application. They should be divided into—A. Preparatory measures. B.—Labour-provocative measures. C.—Labour-accelerating measures. He insisted much upon this division, believing that in many cases no one method could be relied upon, but that it was desirable to proceed in regular gradation through the stages of preparation of the soft passages, the evocation of contractile energy, and the acceleration of delivery, selecting in each stage such means as were most suitable. By the judicious consecutive use of these means we had it in our power to terminate a labour with certainty within a given time, and that with a greater amount of ease and security to the mother and child than had been hitherto attained. This increased perfection of the operation justified the hope that it might be applied to the relief of emergencies and dangers, which had heretofore baffled the skill of the Physician.

[The above is an abstract of a paper read before the Obstetrical Society of London. It induced some interesting remarks from other members of the society. Mr. Baker Brown considers the most successful method of inducing premature labour is to puncture the membranes, and he thinks this proved by the tables laid before the society by Dr. Barnes. The injection of liquids is too tedious and uncertain. He approves of the practice of incising the os in some cases, and considers it may be done when necessary "with safety and advantage."]

Dr. Priestley observed that in determining the safety to mother and child of the several methods of provoking labour, statistics were

particularly valuable; and nothing appeared more conclusive of the advantage of one method over another than the 88 per cent. of children saved by Dr. Hamilton, who separated the membranes, but did not puncture them, as contrasted with the 45 per cent. of Dr. Merriman, and the still smaller per-centage of Dr. Lee, both of whom adopted the plan of puncturing the membranes. Of the various modes which had been suggested, Dr. Priestley had personally found the injection of tepid water into the cervix uteri the simplest and the best. It needed nothing more than a gum-elastic catheter, adapted to a Higginson's syringe, and the operation was almost painless, in most cases easily performed, and usually brought on labour in less than twelve hours. The water so injected had been said to be liable to separate the placenta, and one case had been published in which air had entered the uterine veins during the operation and caused the death of the patient; but it must be recollected that such alarming results were extremely rare, and probably might be entirely avoided by propor precautions. One disadvantage of injecting a large quantity of water into the uterus, according to Cohen's method, until the patient complained of a feeling of distension, had not been alluded to. By distending the lower segment of the uterus and altering the ovoid form of the organ the child was very likely to change its position and present preternaturally. Twice had Dr. Priestley witnessed this. In a case where he induced labour some time ago for Mr. Fergusson, of Notting-hill, the head distinctly presented when the water was injected, but when labour came on the child presented by the feet. Latterly he had taken the precaution to inject not more than four or six ounces of water, which was usually effective, and less likely to alter the presentation. The dilatation by caoutchouc bags might prove serviceable in certain cases, but under ordinary circumstances he was well satisfied with the injection of warm water.

Dr. Hall Davis remarked that in connexion with the Royal Maternity Charity and other institutions during many years, he had frequently been called upon, on account of various indications, to resort to the induction of premature labour, and had employed several of the methods which had been proposed. In the early part of his practice he adopted the original mode—that by the stilet, first performed in 1756 by Dr. Macauley, the colleague of Dr. William Hunter. He (Dr. Davis) made a small aperture, so as to ensure a gradual escape of the liquor amnii. In several cases consecutively this plan was perfectly successful as to the mother, and the children also; but subsequently two cases came in succession, where the children were still-born, which it seemed to him was due to the pressure exerted on the body of the child in the absence of the liquor amnii to defend it. Since the occurrence of those cases he had never, except where profuse hemorrhage had been the indication, resorted to the discharge of the waters for the induction of labour—as a first measure. It should be, he thought, a great object with Obstetricians to save the bag of liquor

amnii for the labour, as being Nature's providential arrangement for safely dilating the genital passages, and at the same time for defending the child from dangerous pressure. His practice now, therefore, was to preserve the waters intact, if possible. He first, if practicable, proceeded by introducing the index finger gently through the os uteri, and upwards outside the membranes, as far as he could reach; he then swept around it, thus dislodging the mucous plug, and separating the membranes to a corresponding extent. He subsequently introduced a piece of soft sponge, previously oiled, as large as could be passed without much difficulty. Where the os uteri would not admit the finger, he commenced with a sponge-tent, supporting it with a piece of sponge at the top of the vaginal tube. He had found these to answer in most cases; in other instances he had resorted to the use of an elastic bougie, passed high up, about half way into the uterus, outside the membranes, previously drawing off the urine, and leaving it in for five or six hours, secured *in sitû* by tapes, so far as to prevent its slipping out again. This he found to answer only in one recent case; labour came on in four hours. He had also employed, and with success, the method usually accredited to Dr. Cohen, of Hamburg, consisting of the injection of water into the uterine cavity. The elastic tube used for the purpose of conveying the fluid to its destination should be passed up cautiously; care must be taken not to rupture the membranes by too forcible injection of the water, to allow it free egress, and also to guard against the injection of air with the water, especially by keeping the end of the suction-tube well immersed in the water to be injected. As regards the induction of labour by the ergot of rye, he (Dr. Davis) could testify that it had the effect of inducing it, but, unfortunately, the children appeared to be destroyed too frequently under its influence. He must say that he was much pleased with Dr. Barnes's paper, and with his proposal of an elastic dilator, water being the medium, as a means of dilating the os uteri, and so of inducing labour. Any addition to the previous resources for the induction of labour, having for its object the saving of the liquor amnii for the useful offices it serves to perform in the act of parturition, must, he apprehended, be considered a great improvement in careful hands.

Dr. Tyler Smith was sure that great good would result from the paper and the discussion which had followed it. The subject itself was most important; and the intrinsic excellence of the paper, and the original proposals which had been made by the author, would do much to attract the attention of the profession to the induction of premature labour. In proper cases it was of the highest value, and its application in practice was still only in its infancy. What had taken place would no doubt lead to its more extended adoption. He did not think that he should have found any one holding a higher opinion of this operation than himself, but he could not quite agree with Dr. Barnes in placing it before the forceps. After the use of the

forceps, he held the induction of premature delivery to be the most important and life-saving of all obstetric operations. He had tried most of the methods of proceeding which had been proposed, and for some years he looked upon the vaginal injections of Kiwisch (which he was the first to use in this country) as far better than puncture of the membranes or any other plan. For the last three or four years he had, however, used uterine injections, and had found them an improvement upon the method of Kiwisch. He had not known of any accident from the vaginal douche, except in a patient whose case had been communicated to him, and who was placed in a cold bath during the douche. In this instance death was caused by internal congestion and hemorrhage. The weak point of the vaginal douche was, that sometimes labour was several days or even a week before it came on. The only death which he had known to follow the uterine douche or injection occurred from puerperal fever, and was not caused by the mode of producing labour. His plan had been to inject about half-a-pint of warm water, of the temperature of the blood, into the uterus, through a catheter or tube passed four or five inches into the uterus. The injection of the fluid should be gentle, care being taken not to inject air. He had never known the coming on of labour to be deferred as much as twelve hours from the time of using the injection, and generally it had commenced immediately. So far there seemed little to be desired. He had never known hemorrhage to be produced by its use. He could not join in the light estimate which had been taken of statistics. These were, in his opinion, of high value, and their collection was one of the most useful objects of the society. He regretted also to hear the light manner in which bold incisions into the cervix uteri, to facilitate labour and ruptures of this organ, had been spoken of. He thought the cervix uteri entitled to greater respect; and he should certainly feel it a grave accident, if, at any time during labour, the cervix were lacerated so as to allow the head to pass through it suddenly. Any new method of inducing labour with safety and certainty would, he had no doubt, receive due attention. He should certainly try the ingenious instruments devised by Dr. Barnes, and should be very glad to find them an improvement upon the injection of the uterus. It might be necessary to caution the profession, who would no doubt try the method of Dr. Barnes, against dilating the os and cervix too suddenly, so as to incur the risk of producing contusion or rupture of the lower part of the uterus, as the amount of fluid pressure which could be brought to bear was enormous. He considered that Dr. Barnes had earned the gratitude of the society by the manner in which he had brought this most important subject before it.

Dr. Barnes, in reply, said, that if it was found that out of 1,000 labours in England five women died, while fifteen or twenty died abroad, that might justify the conclusion that English midwifery was more successful than foreign. But the moment we sought to extract

pathological and therapeutical laws from the compilation of statistical tables, we were surrounded by so many complications and disturbing clauses as to render any conclusions unsafe. If in his bed-side observation he saw that by puncturing the membranes too early the child's life was imperilled by the head being driven against or through an imperfectly prepared cervix uteri, he was convinced of the error of the practice, and was in a position from which he could not be dislodged by the abstracted facts of statistical tables, which could not be subjected to cross-examination and verification. In reference to the method of inducing labour which he had brought forward, he disclaimed the idea of advocating it to the exclusion of others; he rather insisted upon the necessity of observing the three stages he had defined—preparation, provocation, acceleration; and pointed out that, by due observance of this principle and the proper application of the caoutchouc dilator to the cervix, and the improved method of turning, the induction of labour was no longer a matter of uncertainty as to time, but one completely within our control.—*Med. Times and Gazette, April* 27, 1861, *p.* 456.

91.—INDUCTION OF PREMATURE LABOUR.

Case under the care of Dr. Arthur Farre, at King's College Hospital.

The following case of induction of premature labour, for which we are indebted to Mr. John Barr Brown, the resident physician-accoucheur, has lately occurred at this hospital in the practice of Dr. Arthur Farre. The result was attended with complete success.

J. B., a mulatto, aged 38, of very diminutive stature, applied here, during the month of June, for the purpose of having labour prematurely induced. Her previous history runs thus:—She has been married eight years. In 1854, her first labour was terminated at the full time by craniotomy, performed by Dr. Farre. In 1856, labour was brought on, at this hospital, by means of the injection of hot and cold water, the woman being then fully seven months advanced in pregnancy; the child lived only a few hours. In July, 1860, being then about thirty-two weeks gone, she again applied at the hospital, and labour was induced a second time by means of the douche. The operation was commenced on the 28th of July, at four p.m. Uterine pains occurred slowly and irregularly until eleven p.m. on the 29th, when the membranes were forcibly ruptured, which greatly increased the force and frequency of the pains. The child was born at a quarter-past eleven on the 30th, and lived about sixteen hours, during which time it swallowed some milk from a bottle. The weight, when dead, was 3 lb. 2 oz.; it measured in length sixteen inches. The mother had no milk in her breasts. She left the hospital perfectly well on the 12th of August.

In June, 1861, she applied for the third time. Dr. Farre saw her on the 26th. She had then, by her own calculation, finished the thirty-third week of pregnancy. The point of the finger could be easily passed within the lips of the os uteri. Dr. Farre commenced the operation, at three p.m. on the 26th, by introducing an elastic catheter (No. 10 size) for about three inches within the external os uteri. He then swept it round, so as to separate the membranes from the walls of the uterus. This was easily accomplished, except where the head was found to be lying—viz., over the symphysis pubis, a little to the left side. The woman had some slight pains in the back after the operation, but these soon subsided. Uterine pains commenced at six p.m. on the 27th, but were very slight and at long intervals. The membranes ruptured spontaneously at two a.m. on the 28th. The os uteri was now a little larger than a sixpence, soft, cool, and moist. The pains continued slowly and irregularly till five p.m. on the 29th, when they became more effective. At nine p.m. the os was dilated to the size of a half-crown; the presentation was easily made out. The child was born at two a.m. on the 30th, the head presenting in the first position. The umbilical cord, which was looped round the neck of the child, was pulsating strongly. The placenta came away easily about ten minutes after the birth of the child. The child, a female, seemed well formed, and cried lustily. Its head was well covered with hair; the nails were perfect; the pupils of the eyes quite regular; no membrana pupillaris existed: it weighed the next day 3 lb. 4 oz., and measured in length eighteen inches.

From this time, both mother and child progressed most favourably, the latter being fed on milk-and-water from a bottle, as the mother had no milk in her breasts. They left the hospital, on July 13th, both quite well.

This last method of inducing premature labour—viz., by separating the membranes from the uterus by a catheter—is the one specially advocated by Dr. Farre. In the two previous labours the douche was employed, in consequence of the occlusion of the os and the difficulty of passing a catheter.—*Lancet, Aug.* 17, 1861, *p.* 158.

92.—ON THE TREATMENT OF CASES OF ABORTION, IN WHICH THE MEMBRANES AND PLACENTA ARE RETAINED.

By Dr. W. O. Priestley, Physician-Accoucheur to the Middlesex Hospital.

[As is well known, during the first two months the ovum is frequently expelled without rupture of the membranes. But in the later months it sometimes occurs, that, the embryo having escaped, the secundines are not extended, and, after the uterus has made repeated

ineffectual attempts to expel them, the os uteri closes and action ceases. Authorities differ widely as to the treatment to be adopted. Denman, Davis, Ramsbotham, and Dewees, deprecate any attempts at extraction by the hand. Burns and Churchill say "only in exceptional cases"; and Tyler Smith recommends the removal of the secundines in *all* cases. The author has met with a considerable number of patients who have seriously suffered from placental retention.]

The dangers and morbid conditions arising from membranous and placental retention were stated to be:—

1. Flooding; this being sometimes of a very serious character, and the patient being always liable to hemorrhage so long as a fragment remains.

2. Decomposition of the uterine contents, leading to local inflammation of the uterus and surrounding tissues, to phlebitis and phlegmasia dolens, and lastly to general poisoning of the system, as evidenced by irritative fever, peritonitis, rheumatic pains, the formation of abscesses, and even the occurrence of death.

3. Sub-involution of the uterus.

4. The generation of some of the forms of mole out of the tissue left in the uterine cavity, and the hemorrhage attending the presence and expulsion of these morbid growths.

The entire absorption of the placenta was regarded as uncertain and not to be counted upon. Cases illustrative of the several sources of danger were detailed.

In considering the various methods of treatment which have been recommended, the author's experience led him to believe that the effect of the administration of ergot was very uncertain, and that galvanism and injection of water were not to be depended upon. Any form of abortion-forceps which must needs be pushed beyond the reach of the finger he regarded as useless and dangerous. He was thus driven to the conclusion that the introduction of one or two fingers into the uterus was the safest and best way to remove the retained uterine contents. In his hands such manual interference had never been followed by any evil results, and a considerable number of cases had fallen under his observation. He begged particularly to be understood as not advocating rash and violent attempts to empty the uterus. Rough and careless manipulation might be fatal to a patient; but the consequences of placental retention were so serious that, under ordinary circumstances, less risk was incurred by the operation. It was important to effect removal, not only before putrescence began, but before the os uteri became much contracted; and if in six hours after the escape of the embryo the placenta did not follow, it might be removed. The extraction might be effected sooner than this if much hemorrhage were present. He had found it convenient in operations to place the patient on the back, with the thighs flexed on the abdomen; and while one hand steadied and depressed the uterus

externally, the other was with all possible gentleness passed into the vagina. The os uteri was then dilated with the index-finger, and the second finger followed if required. These two fingers formed the best and most sensitive forceps, and although time and care might be necessary to their introduction, their employment was most satisfactory. The administration of chloroform previous to the manipulation afforded great assistance by relaxing the passages and saving the patient from pain. The author had succeeded in removing the placenta with the hand in some cases days and weeks after the escape of the fœtus; but he had never ventured on any such attempt if inflammatory symptoms and irritative fever had already set in. Where the os uteri was too contracted to permit the introduction of the finger for the removal of a placental mass, its dilatation could be effected by sponge tents, which had the double advantage of staying hemorrhage and facilitating the passage of the finger.

[The evil results of these kinds of retained placenta are considerably exaggerated. We almost invariably let nature do the work herself; she will almost invariably throw off the placenta safely sooner or later. Flooding is easily checked by the plug, which, in fact, answers two purposes: it plugs the passage and stimulates the womb to act. We may say all these cases do well if not meddled with too much.]—*Lancet, May* 11, 1861, *p.* 461.

93.—ACCIDENTAL SEPARATION OF THE PLACENTA AT THE SEVENTH MONTH OF PREGNANCY.

By G. E. Stanger, Esq., Nottingham.

Mrs. H., aged 36, mother of three children, healthy, and moderately robust, had arrived at the end of the seventh month of pregnancy, when she took a long walk on the 20th of March, 1861. On her return home, and even before she arrived there, she felt extremely faint, and was scarcely able to walk. She soon became so alarmingly exhausted, that a messenger was sent for me in great haste, as her husband thought she was dying. On my arrival at 3.30 P.M., I found my friend, Mr. Higginbottom, in attendance. The patient was lying prostrate on the bed, blanched, and in a state of syncope. She could only speak in a whisper; but she complained that her body felt very large, hard, and distended. The pulse was very indistinct and fluttering, and her whole appearance indicated extreme danger. She was not aware that she had used any undue exertion, or that she had slipped or injured herself in any way; but I felt convinced that hemorrhage had taken place into the womb, and was in all probability still going on. The abdomen was as large as might be expected at the expiration of the full term of pregnancy. No fœtal sounds could be heard; and the patient remarked that she had not perceived any movement of the child after leaving home in the morning. At this

time there was not the slightest hemorrhage externally; neither was there any indication of the approach of labour; and, on examination *per vaginam*, the os uteri was in its normal condition.

Under these circumstances, both Mr. Higginbottom and myself felt some difficulty in deciding what course we should pursue; for, on the one hand, we feared that if we did not interfere, and the hemorrhage continued, she must inevitably sink; and, on the other hand, if we attempted to induce premature labour, the blood might flow more rapidly as soon as it was allowed to escape from the womb, and death might ensue before we had accomplished the delivery.

We determined, therefore, to wait for a time, especially as there seemed to be now and then an attempt to rally; and we hoped that the os uteri might begin to dilate.

She continued in much the same state during the evening. Mr. Higginbottom remained with her the whole time, as I was only able to call occasionally, in consequence of another engagement.

At 10 P.M., the symptoms not being in any way improved, we decided on the propriety of bringing on labour; but, before doing so, we requested the opinion of Mr. Wright, the senior surgeon to the Nottingham General Hospital. Mr. Wright at once recommended this practice, and considered that the situation of the patient was so critical that it was doubtful whether she would survive the operation. Soon after this advice had been given, hemorrhage began to take place from the vagina in considerable quantity; and the syncope became still more alarming. I, therefore, immediately introduced a straight silver catheter into the uterus, and let off the liquor amnii. By the conjoint and persevering efforts of Mr. Higginbottom and myself, the os was sufficiently dilated to allow of a foot being brought down; but so rigid and unyielding was it that it was a long time before we could succeed in laying hold of the second foot, and even then the exertion required to extract the body was so great that the head was separated from the trunk, which, of course, greatly retarded the operation. We succeeded, however, at length, in removing the head; and we were rejoiced to find that during the two hours that we were engaged in effecting the delivery, very little hemorrhage occurred. The placenta was removed without any difficulty, being already partially, if not altogether, separated. Slight hemorrhage occurred about half an hour after, and great faintness and exhaustion continued for many hours; but the flooding was checked by the application of firm pressure. The patient recovered in a few days without any drawback.

Remarks.—I think it can scarcely be questioned that the plan which was pursued in this case was the only practicable one. Whether the operation ought to have been performed earlier may, perhaps, admit of some difference of opinion. If I were to meet with another case of the kind, I should, as soon as I was satisfied that a separation of the placenta had taken place, puncture the membranes, and then

wait for the appearance of urgent symptoms before I forcibly dilated the os uteri.

The following case will show the danger of delay in such instances.

Several years ago I was summoned to a patient at the same period of pregnancy as the above. She was a person of remarkably lax habit, and had had copious floodings in several of her confinements. On this occasion she had been exerting herself at the garden in pulling up vegetables, and went home very much fatigued. When I saw her, at six A.M., she was very much depressed, tossing about in bed, and bathed in cold perspiration. She said that she felt as if she should burst; and the abdomen was very hard and distended. There was no appearance of hemorrhage externally, but I felt satisfied that blood was being effused into the uterus, and I was convinced that she could not continue long, unless something was done for her. I sent a messenger immediately for Mr. Wright, but she died before he arrived.

I made a *post mortem* examination of the body the same day, in conjunction with my friend, Mr. Thomas Wright. We found the placenta separated throughout its whole extent, and the uterus distended by an enormous coagulum.

In this case, doubtless, the hemorrhage had been going on slowly during the whole night, and there seems very little question that, if the patient had been seen sufficiently early, and prompt measures had been adopted, her life might have been saved. I apprehend that some sudden exertion or strain in these cases produces a slight separation of the placenta; that the blood which is then effused acts as a wedge to detach more and more of it, until in time the whole mass may be separated without any blood escaping from the womb. I do not remember to have met with any account of such cases in obstetric works, and I should be glad if some of the readers of the 'Journal,' would favour us with their experience in relation to this subject.

I think it worthy of remark that no stimulants were administered in the case first narrated. I am satisfied, from my observation of the effects of brandy in cases of flooding, that it tends rather to favour the continuance of the hemorrhage than otherwise, and, therefore, I have for some years entirely discontinued its use in such cases, and I believe that my patients have had not only less hemorrhage, but a more rapid recovery in consequence.—*Brit. Med. Journal, April* 27, 1861, *p.* 440.

94.—THE TREATMENT OF PLACENTA PRÆVIA.

By Dr. Robert Barnes, Physician to the Royal Maternity Charity, London.

[When Dr. Barnes revived the practice of separating the placenta from the lower zone of the uterus, he was careful to point out, that, the separation of the placenta, either partially or wholly, from its uterine attachments, cannot by itself arrest hemorrhage. Contraction

of the womb is the condition upon which the arrest of flooding depends. The great object of all interference in the management of cases of placenta prævia is, to secure this saving contraction of the uterine muscles. Dr. Barnes' views have not been thoroughly understood on these points by many. The report of the first case which follows was drawn up by Mr. Woodman, the resident obstetric assistant.]

"Mrs. F., menstruated last from Sept. 25th to Sept. 30th, 1860. Since that time she has had, especially during January and February, several sharp attacks of flooding, with labour-like pains, especially when she has over-worked. On March 29th labour-pains set in strongly about five p.m., and there was flooding, but not enough to alarm her. As this kept on, however, and the pain increased in severity, they sent for Mr. Holman, the gentleman appointed to the case. On his arrival, he found a pool of blood on the floor, and the woman almost fainting. He sent for me immediately. When I came I found she had lost a great deal of blood, at least thirty or forty ounces, as there was a pool on the floor, and the linen of the patient, bedclothes, &c., were all soaking with it. On making examination, I found a portion of the placenta presenting externally, and nearly separated from the rest. No one had pulled it, for the women were afraid, and Mr. Holman thought it best to send for me, as it was so near the hospital. I removed this loose piece, and on making a digital examination, found that the placenta was mostly in the vagina, except a portion which was firmly adherent to the uterus, just within and above the anterior lip. I could also feel a small fœtal head in the usual position. The os was dilated to about the size of half-a-crown, but was easily dilatable by the finger. Pains had ceased, and flooding was still going on. I loosened the attached portion of placenta, and removed it entirely, which checked the hemorrhage completely. I then gave about a drachm of ergot powder in a little warm tea, which brought on pains again, and the fœtus was expelled in about fifteen minutes, with about three pains. No hemorrhage followed. The funis broke off in removing the placenta; it was very soft. The placenta was easily lacerable, and appeared to have been broken up by the natural efforts at expulsion. The child was very œdematous. I need not further describe this or the placenta, as both have been seen by Dr. Barnes."

If we analyse this case, we shall find several points of interest. In the first place, we observe that there was profuse flooding in connexion with a dead fœtus, for there is no doubt that the fœtus in this case had been dead some time before the labour set in. This disposes of one theory that has been advanced—namely, that the occasional spontaneous arrest of hemorrhage is due to the circumstance that in such cases the child has been dead some time, and the ultero-placental circulation arrested previously to the supervention of parturition. This theory accords indeed with the doctrine of the placental source of hemorrhage, but it does not accord, as the case before us proves, with

clinical observation. The simple fact is, that the hemorrhage did not stop until the womb contracted, although nearly the whole of the placenta was detached. When the portion adhering to the cervix was removed, contraction was excited, the flooding ceased, and the child was *expelled* in about fifteen minutes. The expulsion of the child is an unmistakable sign of active uterine contraction. This contraction, just as it does in post-partum hemorrhage, arrests the bleeding by constricting the mouths of the vessels.

I will relate another case in illustration of this fundamental principle.

Case 2.—On the 26th of April last I was called by a midwife of the Royal Maternity Charity to a woman who had been flooding profusely. She had borne four children at term. She had now arrived at the end of the eighth month of pregnancy. Flooding set in for the first time at ten p.m. on the 25th, and continued more or less during the night. The midwife saw her at three a.m. on the 26th, and I arrived at half-past four a.m. I was shown a large chamber vessel half full of clotted blood, and this was by no means all she had lost. The pains were very slight. The patient was weak, but not greatly exhausted. The os uteri was high in the pelvis, and was reached with difficulty; it was expanded to the size of a half-crown. The placenta was felt extending over the os internum, but the membranes could be felt anteriorly. I swept the forefinger all round the lower zone of the uterus, separating the placenta. Then I punctured the membranes with a steel pen, and holding the os open, whilst firm pressure was exerted upon the uterus externally, liquor amnii freely escaped. A bandage was then applied to the abdomen, to maintain firm compression on the uterus. By these means the capacity of the womb was much diminished; the muscular structure contracted in adaptation with the lesser bulk of the contents, and no more bleeding ensued. At this time it would have been indeed a concession to authority, but a violence to Nature, to have proceeded to forcible delivery. Things appeared to be now favourable for the completion of labour without the further intervention of art. The woman was delivered normally of a living child at one p.m. without further hemorrhage.

What do we learn from this history? Or let me first ask, what might we have learned, if, following ancient precepts, I had, during the impetus of the flooding, delivered forcibly by turning when first called in the morning? One thing we should not have learned: we should have excluded the possibility of observing the process by which Nature checks hemorrhage in these cases; we should have raised another barrier against the admission of true knowledge, receiving only confirmation in the prejudice that Nature is here utterly at fault, and cries aloud for the instant help of the physician. But something positive we might have learned too. In the first place, with a cervix uteri only partially dilated, firm, and high up in the pelvis, turning would not have been an easy operation; to drag the child through such

a cervix is to afford it but a slender chance of life. In all probability, instead of a living child, we should have a dead one. In the next place, great violence would have been done to the cervix uteri, and this violence would perhaps have been revealed by the occurrence of secondary hemorrhage, the result of laceration of the cervix, and, at a later period by the development of uterine inflammation and pyæmia. Let me recall to your attention the following considerations. The normal slow and gradual opening of the cervix uteri under the natural influences of contraction of the uterine muscles and hydrostatic pressure is not seldom a painful operation. Whosoever has had the opportunity of dissecting the genital organs of puerperal women who have died within a few days after even an easy labour, will have witnessed an amount of contusion, of ecchymosis, perhaps of laceration, of the lower part of the cervix uteri which will satisfy him as to the hazard of forcibly stretching open that structure with the hand. But in a case of placenta prævia this danger of injury to the cervix uteri is enormously increased. Instead of structures in a normal condition expressly adapted to undergo with safety the distension of labour, we have before us structures preternaturally developed under a misplaced physiological stimulus; the cervix uteri is now full of large vessels, which when the placenta is detached, expose open lacerated mouths on the surface. Through this highly developed tissue, and over these torn, gaping vessels, the child must pass. Nor can it always pass with impunity, even when the delivery is accomplished by the regulated efforts of Nature. Occasionally, and under apparently most favourable conditions, the bruising of the hypertrophied cervix and the injury to the vessels are followed by fatal hemorrhages or pyæmia. It must not be forgotten that the lochia, and all the discharges, healthy and unhealthy, from the cavity of the uterus, must flow over these bruised open vessels. It demands all the care and all the skill of the obstetrist not to add to this terrible risk. Yet surely the irregular bony fingers of the surgeon cannot, however tenderly he may use them, effect the distension of the cervix uteri without adding to this risk. This is what the *accouchement forcé* might have taught us. But you will say, there was another resource. Instead of turning, the placenta might have been wholly detached. No doubt it might; and I will even admit that the hemorrhage might have stopped. It would have stopped, however, not because the placenta was separated from the uterus, but because the operation would necessarily have caused the discharge of the liquor amnii. But this object was accomplished quite as effectually by simply puncturing the membranes with a steel pen. And I will ask you if you believe it possible to separate the entire placenta when it adheres, as it so commonly does in these cases, to a very large proportion of the area of the uterus, extending six, seven, or eight inches from the os internum, without passing the whole hand into the cavity? And if you had resorted to this proceeding, you would have actually used more

violence than if you had turned; for turning can be effected without passing more than two fingers through the cervix.

Passing from the dangers we might have experienced had the woman been forcibly delivered, or the entire placenta separated, let us now see what the case, treated on true physiological principles, teaches us. First, we see the flooding persisting so long as there are no effective pains, and no contraction; secondly, we see the flooding arrested so soon as the placenta is detached from the lower zone of the uterus, and the liquor amnii discharged; thirdly, we then see the labour progressing under the unaided powers of Nature, and ending in the birth of a living child, and the safety of the mother. Not interposing, *nimiâ diligentiâ*, a screen of our own construction, we are enabled to detect another fallacy that has long hindered the recognition of the true physiology, and consequently fostered an erroneous treatment, of placenta prævia. This case tells us plainly, that the dogma which affirms that the hemorrhage increases with every successive detachment of placenta, and in proportion to the extent of the detachment, is not true. The flooding ceased when all that portion of the placenta which grew to the lower zone of the uterus was designedly detached by the finger, contraction, be it borne in mind, concurring; for without contraction the flooding might still have gone on. The remaining part of the placenta, being connected with the lateral and fundal zones of the uterus, the region of normal attachment, was safe from the risk of detachment. There it remained to maintain the life of the child until committed to aërial respiration.

But all cases will not terminate so well unless further aid be given. The cervical detachment of the placenta and the evacuation of the liquor amnii may not be sufficient to induce full hemostatic contraction of the uterus. Great loss of blood not uncommonly tends to paralyse the uterus, so that it will respond neither to pressure, to ergot, to brandy, or any other oxytocic agent, so long as its walls are distended by the presence of the fœtus. Or the enfeebled powers of the uterus may be spasmodically exerted and perverted, owing to the abnormal condition of the cervix. Under these circumstances, the full and uniform contraction of the uterus, the true hemostatic process, cannot be ensured until the organ is emptied. And even although no further hemorrhage may occur, the exhausted patient rarely completely rallies whilst she remains undelivered. But from the moment that, labour completed, the uterus is fully contracted, the systemic and mental reaction is surprisingly rapid. Depressing anxiety gives place to confidence; and confidence is the parent of security. Having done what you can to persuade Nature to help herself, you may find it necessary to complete delivery by artificial means. But still, even under this emergency, there is no need of precipitation or of violence. It is not necessary to force the hand through the cervix, either to tear away the whole placenta, or to extract the child. We possess other means of accelerating and completing the delivery, which possess

this great recommendation, that they themselves bring no element of danger to add to the inherent perils of the case.

I will relate another history to illustrate the treatment to be pursued in these more formidable cases.

Case 3.—On Nov. 19th, 1860, I was called to a woman who had borne several children. She was in her ninth month, and had been losing blood freely for a week, with slight intermissions. Flooding came on with increased copiousness the previous night. I saw her at four A.M.; she was much prostrated; had yawned, and tossed her hands, and was very faint and anemic. The placenta was quite over the centre of the os. The cervix was more than an inch long. The os externum uteri was sharp, the size of a half-crown. There was little bleeding at the time of examination, and no pains. But as I feared the bleeding might return, and saw that the patient was too exhausted to bear further loss, I considered it necessary to expedite delivery. I therefore detached the placenta from the lower zone of the uterus, and then inserted a caoutchouc dilator into the cervix. By injecting water into this bag, the cervix was freely expanded in about five minutes; but pains were not excited. I was, however, enabled to pass my hand by the placenta, and reach the membranes, which I ruptured with a skewer, there not being tension enough to break the sac with the fingers. The head was presenting. A foot was easily seized. I ascertained that neither cord nor heart was pulsating. The child was extracted in a few minutes: it was premature and still-born. The uterus being now made to contract by pressure, the placenta was speedily cast and withdrawn. No hemorrhage followed the labour. The patient recovered well.

In this case had I not possessed the caoutchouc dilator, I should have been compelled to stretch open the cervix uteri with my fingers at the risk of injury, or to encounter the alternative risk of renewed flooding and a lingering labour, with exhausted uterine power. By accelerating labour, the patient was quickly placed in security.

To sum up: we have, you will perceive, an ascending series of remedial measures finding their application successively with the rising obstinacy and dangers of the case.

1st. Detach, with one finger passed through the cervix, all that portion of the placenta which adheres to the cervical zone of the uterus.

2nd. Tap the amniotic sac, and drain off the liquor amnii. To effect this, some change of posture on the part of the patient—holding the cervix a little free from the presenting head, which sometimes acts like a ball-valve, completely closing the os—and firm pressure on the uterus externally, will sometimes be necessary.

3. If the uterus still remain flaccid, and if hemorrhage continue, or, generally, where the patient is much exhausted, so that effective muscular contraction cannot be roused, artificial aid is called for in order to accelerate delivery.

This necessity may arise under two conditions. First, the cervix may be freely expanded. If this be the case, you may immediately turn the child by passing two fingers of the left hand through the cervix, gently yet steadily tilting the head forwards over the pubes, whilst simultaneously pressing the breech backwards and downwards, until a knee or foot comes over the os within reach, and then hooking this down into the vagina. When the breech is engaged in the cervix you must extract slowly and with care. The breech will thus fully prepare the cervix for the quicker and safer passage of the trunk and head, increasing the chance of life to the child. All this time steady pressure must be kept up on the uterus externally.

But, secondly, you may have an undilated cervix, with more or less rigidity. This is the condition in which the coautchouc dilator offers such invaluable service. A bag of suitable size and shape is rolled up and slipped into the cervix either by the fingers or by aid of a stem; it is then to be distended with air or water, so as to put the cervix gently on the stretch. This will act in three ways: it will mechanically stretch the cervix just as the natural bag of membranes will, when driven down strongly by vigorous contractions of the uterus; it will press upon the mouths of the vessels and tend to arrest the hemorrhage; it will, again imitating the distended amniotic sac, excite the diastaltic function of the uterus by stimulating the incident nerves distributed over the os internum.

In this manner the energies of the uterus may be roused sufficiently to terminate the labour without further help. If this should not be the case, then you may expect, in a short time, to find the cervix so dilated as to admit of turning in the manner already described. I assume that, throughout, all care has been taken to support the patient by nourishment and stimulants.

Sometimes the removal of the placenta will be tedious. There are three conditions which may impede the expulsion of the placenta: first, there is the atonic condition of the uterus, so frequently present in these cases, giving rise to actual paralysis, or to spasmodic and irregular contractions: secondly, the uterus is less able to cast off placenta which adheres to its lower zones; and, thirdly, there is a great liability to morbid adhesions. I have, I think, accumulated sufficient evidence to prove that a diseased state of the uterine mucous membrane is a predisposing cause of placenta prævia; and out of this diseased mucous membrane the decidua is developed which is to form a constituent part of the placenta, and to bind this organ to the uterus. If no morbid adhesions exist, you will rarely fail to induce the uterus to thrown off the placenta, if you grasp it firmly for some minutes through the walls of the abdomen before you begin to pull upon the cord. If adhesions exist, it may be necessary to separate them with the hand. A firm binder is of especial service in these cases.

If you bear the leading principle well in mind, that contraction is

the end to be secured, and if you bring into successive use the measures I have described, you will, I am confident, experience more success in the treatment of this, one of the most dangerous complications in midwifery, than has been hitherto arrived at.

The majority of the deaths in placenta prævia arise either from hemorrhage or from pyæmia. Hemorrhage you may greatly avert by accelerating the labour by the judicious resort to the separation of the placenta from the lower zone of the uterus, by rupturing the membranes, by pressure externally, and, above all, by the intra-uterine application of the caoutchouc dilator. Almost all the violence which is the most frequent cause of pyæmia, you may avoid by these accelerative measures, and by turning when the parts are duly prepared by combined internal and external manipulation.—*Lancet, June* 1, 1861, *p.* 527.

95.—OBSERVATIONS ON PLACENTA PRÆVIA.

By Dr. Charles Clay, late Senior Medical Officer to St. Mary's Hospital, Manchester.

[No form of uterine hemorrhage more especially calls for the energies and resources of the practitioner than that arising from placental attachment to the os uteri, and consequently any observations on the subject must be received with attention by the profession.]

In 1822, Kinder Wood, Esq., of the Manchester Lying-in Hospital, with whom I commenced my own professional career, had observed cases in which the placenta was attached wholly or partially to the os uteri, and in which, when the placenta chanced spontaneously to separate, immediate cessation of the hemorrhage followed, both child and placenta being subsequently expelled by the simple efforts of nature unaided by the accoucheur, and without any bad consequences, the patients generally doing very well, and making reasonably rapid recoveries. From these facts he inferred, that by simply detaching the adhered placenta from the os uteri by the forefinger, the case might safely be left to nature for completion in almost all cases; and that the result would be far less dangerous than the usually accepted plan of version, which is not always applicable without a great amount of violence, and even in its most desirable view is attended by a large amount of fatality, even when unaccompanied with the extensive hemorrhage that usually characterizes cases of placenta prævia. One of Mr. Wood's strongest points of argument rests on the *now well-ascertained fact* that in all cases, immediately after the detachment is effected, the hemorrhage *as certainly ceases*. Another point equally forcible is that detachment can be accomplished at much earlier stages of the dilatation of the os uteri—when only the finger can be admitted—and before any great prostration can have taken place, and this at a time when it would be the height of imprudence, (which is the mildest

possible term that could be applied to such a proceeding) to forcibly dilate an unwilling and almost undilatable os to the extent of introducing the hand into the uterus with the view of version. The amount of violence done to both mother and child is excessive. A point abundantly proved and confirmed over and over again by the high rate of mortality displayed in the statistical returns of various authors. Indeed the per centage of mortality shown on version *in all cases is so very high*, that, without the necessary increase applied to those cases where extensive hemorrhages accompany it, it cannot be justifiable to increase the mortality if any possible means can be suggested for its reduction, and which I trust will be clearly shown in these remarks.

Mr. Wood's plan of detachment was not only proposed, but put into practice for a considerable period before 1822, and with invariable success; an example that was not lost on his pupils, myself amongst the number. I have, for nearly forty years, continued the the same practice with almost entire success, except in one or two solitary instances, where the distance travelled was great, and consequently the loss of blood, and time, had produced an unconquerable amount of prostration. In 1845 Professor Simpson of Edinburgh, whose indefatigable exertions for the improvement of the profession are beyond all praise, again opened the general question, and a lengthy controversy arose between him and Dr. Radford of Manchester as to priority of views. Mr. Wood's previous claim was almost lost sight of during this controversy; and this was the more remarkable as Dr. Radford had been the colleague of Kinder Wood for many years, and was perfectly well acquainted with his mode of practice.

In cases of placenta prævia, repeated examinations cannot be too severely condemned. The one examination to ascertain the facts of the case should, if possible, be followed by prompt and energetic means to check the hemorrhage by detachment, and thus facilitate subsequent delivery by the efforts of nature only. If the hemorrhage ceases on the detachment of the placenta, the delivery may with great propriety be waited for, and without danger. I have scarcely known an instance to the contrary. In reference to the old plan of version and immediate delivery, for which the detachment of the placenta is proposed as a substitute, there is in the first place the heavy rate of mortality of 1 in 3 to the mother, and 1 in 2 to the child, increased considerably if those cases are treated separately. Where the turning and delivery are effected when the dilatation of the os uteri has only just begun, that is, only to admit the point of one finger, or to within the size of a shilling at most—a rate of mortality would be shown which I do not hesitate to place at near 50 per cent. to the mother, and almost universally fatal to the child. These figures have to be compared with 1 in 44 to the mother, and 1 in 5 to the child, where the placenta is detached. Secondly, there is the probability of means having been previously tried to check the hemorrhage by rupturing the membranes, or giving ergot, *or both;* either of which will

immensely increase the difficulties and dangers attendant on subsequent version. Thirdly, if the os uteri is but very slightly dilated and not disposed to do so, the violence done by attempting to turn is very serious and the difficulties of version increased. Fourthly, if the hemorrhage occur before the period of utero-gestation is completed (the seventh month for instance), the os is certainly not prepared for *extensive* dilatation, and I do not believe it ought to be subjected to such extreme treatment. Fifthly, the danger of waiting for a sufficient state of dilatation to effect version judiciously (say to the size of half-a-crown, as generally admitted by the best authorities), and the great loss of blood in the interim, when the hemorrhage can be at once checked and the waiting for natural expulsion justified, by the simple process of detachment. Sixthly, in almost all cases the prostration is often so great that even the necessary efforts of version will in very many instances hasten death.

I am convinced from long experience, that the dangers of version and hemorrhage may in a great measure be done away with by the simple detachment of the placenta. I have never known it to fail, nor do I believe it will ever fail, if the detachment is completely and properly effected. We have also the highest authorities for stating that the arrest of hemorrhage is complete in 19 out of 20. If even then for the sake of argument we admit one failure in twenty cases, the results of detachment would stand immeasurably superior to the old (and I trust soon to add obsolete) system of version and immediate delivery.

It has been objected to detach at the seventh month, the os not being dilatable; but I have found it as easy dilatable at the seventh as at the ninth month, I mean *so far* as is necessary for detachment only, and therefore believe the objection to be more ideal than real. Then again, if it be an objection, how much more objectionable must it be to dilate more extensively, as in version and delivery, where the violence must of necessity be greater. Others argue that the fœtus may be retained for days after the detachment, and fever may arise, but as no facts have been advanced to confirm such an opinion, it can only be entertained as a mere supposition. I have never seen the fœtus retained, and consequently no case of fever from such a cause has ever come under my notice, and certainly it does not apply to cases of fœtus dead *in utero* from other causes. After all, there is such a remarkable difference in the statistics on these important questions, that I should consider any person reckless indeed of human life, who would advocate turning and delivery in cases of placenta prævia in preference to detaching the placenta, if at all aware of these results. But I think few could be found to go further than either of these doctrines, and advocate forcible dilatation at early periods, or attempt turning with the os only capable of receiving the point of the forefinger, or at most dilated to the size of a shilling; increasing the already too great mortality, and adding greatly to the future difficulties to be contended with. One other point has been advanced—that

detaching the placenta encourages indolence of practice, and screens inability. I would only observe in answer, that to watch such a case *as it ought to be watched*, is anything but indolence; and to detach a placenta neatly and effectually, so as to realize our full expectations, requires as skilful and as careful manipulation as version. And if the accoucheur can accomplish the former, there is no fear of his being able to manage the latter.

In conclusion. I have never witnessed any bad consequences from detaching the placenta; there is infinitely less violence done, the danger is much reduced, future difficulties are of less importance, and the results far more favourable. And with the accumulated facts of the past forty years, from individuals of the highest standing in the profession, we may safely hope never so far to retrograde as to adopt the old and barbarous system of boring through the placenta, turning, and delivering the child. But even if we should be so far led astray as to accept this old barbarism, let us at least escape the opprobrium of attempting such practices in the earlier stages of dilatation of the os uteri, and *knowingly* increasing all the dangers attendant on such cases.—*Glasgow Medical Journal, July* 1861, *p.* 129.

96.—ON A PECULIAR (SYPHILITIC?) DEGENERATION OF THE DECIDUA.

By Professor Virchow.

Professor Virchow recently examined the membranes of a fœtus from two to three months old, which had come away from a woman who had been healthy before her marriage. but had soon after it become hoarse, and suffered from syphilitic ulcers of the throat. These were cured by the protioduretum mercurii; she then conceived and had a miscarriage soon afterwards. The fœtal part of the membranes showed an unusually strong development of the cells of the chorion at the placental part, which looked like cylinders and were covered with long and fine epithelial cells. The maternal part of the membranes was even more changed; it had come away quite separately from the ovum itself. The whole inner part of the mucous membrane of the uterus (the decidua vera) had become loose, and only that part which covered the fundus was wanting. The change was especially striking at the posterior and anterior part of the inner surface of the decidua, where it was very thick, from one to four lines, and was covered with large, polypous growths, part of which were half an inch long, a quarter of an inch and even more broad, and three-quarters of an inch high. Their insertion was broad, and they became narrower towards the top, which was bluntly rounded. Between these growths the decidua was smooth, a little wrinkled, and of a pale whitish colour. The orifices of the utricular glands were only with difficulty recognised. The whole of the external surface was rough, almost villous.

The large tuberosities, which were similar to papulæ and tubercles, had a smooth and dense surface of a red colour. A hyper-plastic state of the mucous membrane evidently existed, which was also confirmed by the microscopic examination; for M. Virchow found a foundation layer in which fibres were recognised, and large cells of lenticular form, which on a perpendicular cut looked like thick spindles. This hyper-plastic state was no doubt due to irritation; it was an endometritis papulosa and tuberosa, which had probably caused abortion. It is difficult to say whether the endometritis was of a syphilitic nature, and whether the papulæ and tubercles were to be considered as condylomata. But this is very probable, as M. Virchow has found in the mucous membrane of the uterus of syphilitic women sometimes symmetrical, sometimes truly papulous swellings.—*Medical Times and Gazette, June* 8, 1861, *p.* 610.

97.—WHAT INFLUENCE HAS CONTRACTION IN STOPPING UTERINE HEMORRHAGE.

By Dr. James Adams, Great Wakering, Essex.

[In Dr. Barnes's lecture on placenta prævia (see page 241 of this volume.) the opinion generally held by medical men is expressed, viz.—that the hemorrhage which follows the separation of the placenta is stopped by the contraction of the uterus, causing compression of the vessels, and thus preventing the escape of blood.]

The contraction has without doubt great influence, but I do not think that it entirely accounts for the phenomena. I have found in more than one case that on adopting Dr. Barnes' plan of partially separating the placenta the hemorrhage has ceased, although the bulk of the uterus has not materially diminished. For the contraction to be so perfect as to close the vessels, the circulation would be so arrested in the attached portion—a part of the supply being already cut off—as to render the birth of a living child utterly impossible. In Dr. Barnes' second case the child was born alive several hours after the partial separation of the placenta, and consequent arrest of flooding. In this case the liquor amnii was discharged, and the uterus contracted on the child; but still the size of the uterus was very great, and the contraction was not permanent, but followed by relaxation. What is to prevent the flow of blood during the relaxation? The vessels would still be very large, and the pressure of the child's head could not act altogether as a compress, as a portion of the placenta was interposed between the head and the surface of the uterus.

A more reasonable explanation of the arrest of hemorrhage appears to me to be that there exists in the uterine vessels, as in arteries generally, a power of contraction and retraction; for we find that the flooding ceases at once on the violent separation of the placenta, but

not when it is gradually and gently separated, either by the enlargement of the uterus and disappearance of the neck, or by uterine contraction, as in cases where labour has begun and the os is somewhat dilated. Two cases of this nature have fallen under my notice, the arrest of hemorrhage having been effected in one by the manual separation of the placenta, and in the other by turning and delivery. That the power of contraction and retraction does exist in the vessels of the uterus is certain, or how would they become lessened with the diminished size of the uterus as labour becomes completed? They would in such a case become folded upon themselves. If they possess the power of contraction and retraction, why should it not be exerted for the purpose of stopping hemorrhage? That Nature should have endowed them with inherent power for effecting so important a purpose is only what we should expect, considering how beautifully perfect she is in all her operations.—*Lancet, July* 6, 1861, *p.* 23.

98.—ON THREE CASES OF PUERPERAL CONVULSIONS.

By Dr. Robert Harper, Holbeach.

Case 1.—A. M., aged twenty-three, a stout, healthy-looking young woman, at full period of gestation; first pregnancy.

Feb. 6th, 1858. Eleven A.M. I am summoned hastily to see her in consequence of her having had several fits. When I arrived (a distance of two miles), I found that the convulsions followed up more quickly, and that stertor and deep insensibility existed between the attacks, whereas at first she was perfectly conscious during the intervals. The convulsions were so powerful that it took from six to eight persons to hold her. Her tongue was much lacerated in consequence of the attendants not having put anything between her teeth during the fit. The os uteri was not dilated; pulse full, hard, and bounding. I bled her to about sixty ounces; and although the pulse was not sensibly altered, I was afraid to go on. Croton oil and a large dose of calomel were given; castor oil and turpentine enemata administered; the hair was cut off, and cold lotions applied to the head. About three hours afterwards the bleeding was repeated to about twenty ounces, and a large number of leeches applied to the head. The croton oil and calomel were also repeated.

7th. Convulsions, stertor, and comatose condition the same; the bowels freely and repeatedly acted upon; the os uteri dilated, and head down in the pelvis. She was delivered of a dead child by the forceps; the placenta easily extracted. A blister was applied to the neck, and mustard plasters to the calves of the legs.

8th. Convulsions gradually became less frequent until they ceased altogether about noon. Complete consciousness did not return till after their total cessation. From this time she gradually improved, and got quite well. She has had two living children since.

Case 2.—A. B., aged twenty-one, a stout girl; first pregnancy; full period.

May 7th, 1859. Labour commenced last night at eleven A.M. She was progressing favourably, and I left her in charge of my pupil, Mr. J. Morton, for a short time. At two P.M. she was seized with a fit, soon followed by others; stertor and insensibility between them. Pulse firm and bounding. The head being in the pelvis, I delivered her with forceps of a living child. The placenta was quickly thrown off; soon after which flooding, to a most alarming extent, occurred, and which was restrained with difficulty by the introduction of the hand into the uterus, and pressure, combined with cold applications, to the abdomen. The convulsions and deep insensibility continued for more than forty hours, and although considerable abdominal tenderness followed, she made a good recovery.

The same treatment was adopted as in the preceding case, except that venesection and leeching were not used.

Case 3.—H. F. M., aged twenty-four. About ten months since she was rather delicate-looking, but has become much more robust the last few months. Primipara, full period.

April 29th, 1861. Was delivered of a living child at three A.M., after only three hours' labour. Placenta thrown off naturally; bowels acted freely during labour from a dose of castor oil taken the previous evening. Two P.M.: Progressed most favourably up to this time, when I visited her. She was at first asleep, and upon awaking she appeared alarmed; did not answer questions put to her; seemed to have a choking sensation in her throat; then a violent winking of the left eyelids came on, and directly afterwards a convulsive fit. Stertor and total insensibility immediately followed. The convulsions recurred every twenty minutes throughout the remainder of the day. Calomel and croton oil were administered by the mouth; the hair cut off, and cold applied to the head; bleeding from two veins at once to between thirty and forty ounces.

30th. Three A.M.: Much the same. Several leeches were applied to the temples, and mustard plasters to the calves of the legs; bowels acted freely. Eight A.M.: Has only had one more fit; stertor less, but insensibility the same; bowels acted several times. One P.M.: The same, except that the pulse is 140, and not so full. I examined the abdomen carefully, but could not ascertain that there was any tenderness or tumefaction. Six P.M.: Patient quite conscious; pulse 140, small and moderately firm; complains of pain and tenderness over the lower part of abdomen. Ordered calomel in small repeated doses; leeches to painful part of abdomen, followed by poultices.

May 1st. Much the same, except that there is tumefaction of abdomen; no more fits; sensibility complete. About two hours after this she became much altered, and gradually sank at twenty minutes to four P.M.

Remarks.—I have sent the foregoing very briefly written cases for

publication, because it is unusual for three such cases to happen in one medical man's general practice in so short a time; secondly, they each differ as to the period at which the convulsions commenced—namely, before labour began, during labour, and eleven hours after its completion; and lastly, it will be interesting to hear the opinions of other practitioners as to the most desirable and efficacious treatment of one of the most formidable and frightful complaints which a medical man ever has to treat or witness, entailing upon him much labour, thought, and anxiety, and, in the case of a fatal termination, often much opprobrium. In the first case which I have related, I was afterwards told by the patient's friends that if she had died, they should *not* have attributed her death to the disease, *but to my excessive bleeding*. In the case of A. M. and A. B. there was evidently a plethoric condition previously; but as I knew nothing of them till called in at the last moment, this did not come under my observation. In the case of H. F. M., she was at all times highly nervous, having a *great dread* of seeing anybody in a fit (which from the office she held she was liable to), and the convulsions were inmediately preceded by an appearance of alarm, the patient, as it is termed, "awaking in a fright."

In conversation with a very intelligent medical friend, I was told by him that an eminent obstetric lecturer to one of the leading London medical schools repudiates the bleeding plan of treatment now for some years almost universally adopted in this complaint. If such be the case, I shall be glad to know if other teachers promulgate the same views, and the amount of success which has followed the new system.—*Lancet, June* 22, 1861, *p.* 607.

99.—USE OF CHLOROFORM IN PEURPERAL CONVULSIONS.

By Dr. Fred. Page, Landport.

On Feb. 14, Mrs. S., a woman about 30 years of age, in every respect healthy, and naturally of a cheerful disposition, at the full period of pregnancy with her sixth child, was suddenly seized with convulsions about mid-day. When I saw her, she had had seven or eight severe attacks in an hour. She was lying in a state of coma, with stertorous breathing, contracted pupils, pulse full, strong, about 80. She had a severe paroxysm soon after I arrived, with violent struggling, foaming at the mouth, &c., which left her, as before, in a state of lethargic sleep.

The os uteri was intact; there was not the slightest disposition to dilate; nor was there any symptom of approaching labour. She was bled to sixteen ounces. A violent fit ensued. I then kept her under the influence of chloroform for half an hour, and waited another half an hour; no fit occurred. A turpentine enema was given; and a blister was applied to the neck.

At six P.M., she had had convulsions at intervals. At the moment of my entering the room, the child was expelled suddenly during a fit. The placenta followed at once, and the labour was complete. The patient was still insensible, with rapid convulsions. The emena operated well. I again used chloroform for half an hour.

10 P.M., four hours after labour. The patient was very quiet, sleeping: there had been no return of the convulsions.

Feb. 15, 8 A.M., sixteen hours after delivery. She had been in convulsions during the night. I found her at 9 P.M. much as I left her the night before, in profound sleep. She had convulsions whilst I was there. Chloroform was again given for half an hour; after which she had no attack. Sensibility gradually returned, but it was some days before she quite recovered. She and her child are now quite well. I may remark that she has not the slightest recollection of anything that happened during her illness.

Remarks.—This was a case which assumed what is called the apoplectic form, in contradistinction to the hysteric, and in which much doubt has been expressed as to the propriety of exhibiting chloroform. In reviewing this case, it is evident that chloroform checked the attacks. It was used at three distinct periods: 1. Before delivery, preventing them for two hours; 2. Directly after delivery, for four or five hours; 3. Sixteen hours after delivery, when they ceased.

The first anxiety in puerperal convulsions is to empty the uterus and bowels as early as possible; but convulsions continued for twelve hours after the former had taken place. I must conclude that bleeding, the use of chloroform, and early emptying the uterus, are the grand remedies in this disease—especially chloroform. I shall be glad to hear more of its use in these cases.—*British Med. Journal, April* 13, 1861, *p.* 386.

100.—ON A CASE OF EPILEPTIC PUERPERAL CONVULSIONS.

By Dr. F. W. Wilson, Medical Officer to the East Grinstead Union and Workhouse.

[This case is interesting from the fact of chloroform having exerted a marked influence in arresting the fits, both during and after labour. The patient was eighteen; primipara in the eighth month of pregnancy. The fits commenced early in the morning, and continued with increasing severity during the day. In the evening her condition is reported as apparently desperate.]

The fits were occurring every quarter of an hour, and were extremely violent. The respiration in the intervals was effected with the greatest difficulty; indeed, in the state she then was it seemed both to Mr. Whyte and myself impossible for her to exist more than two or three hours. As labour had somewhat advanced, the os uteri being nearly

the size of a half-crown and very dilatable, it was determined to deliver artificially; but on account of the violence and frequency of the fits we also determined to use chloroform cautiously ere doing anything. I placed half a drachm in the inhaler and held it to the face; the air was drawn in with so much violence that in half a minute considerable effect was produced, the pulse being scarcely perceptible, and catching of the breath occurring, which caused us some alarm. We were neither of us prepared for the most favourable effect which afterwards, however, ensued, as in ten minutes from the application of the chloroform the stertor was much diminished, the pupils became somewhat under the influence of light, and our patient was lying as placid as when I saw her in the morning; she had not breathed so easily, indeed, since the commencement of the fits. In addition to these good effects, labour advanced with regularity, and by applying the forceps it was completed in the space of about half an hour, only one fit occurring during the time, for which the chloroform was again applied with good effect. Hour-glass contraction occurred, which rendered it necessary to introduce the hand to remove the placenta.

Having remained an hour afterwards with my patient, and no fit occurring, I left, desiring to be sent for in the event of an unfavourable change. Shortly after my leaving a severe fit again came on, and in an hour's time another. When I reached her (about three hours after delivery) I found the stertorous breathing as bad as ever. Another severe fit shortly supervening, I again administered chloroform, after which the breathing was tranquillized and no fit occurred for some time. Once afterwards, chloroform was again given with apparently equally good effect, and after this only one slight fit occurred. *Lancet*, *May* 4, 1861, *p.* 432.

101.—ON SUPPORTING THE PERINEUM IN LABOUR.

By Dr. Graily Hewitt, Physician to the British Lying-In Hospital; Lecturer on Midwifery and Diseases of Women and Children at St. Mary's Hospital Medical School.

[Dr. Hewitt thinks that the principal effect actually produced by supporting the perineum is retarding the advance of the head, and even this effect cannot be obtained unless by the exercise of a considerable degree of force.]

It appears to me that the only conclusion we can come to is, that the operation must be rejected. Its object, if that object be limited to retarding the advance of the head, may be in itself good; but this particular method of attempting to gain that object is objectionable. In ordinary cases of labour, as a device for preserving the perineum from laceration, I believe it to be practically worthless, and there are grave reasons for surmising that in many cases it has led to the very evil it was intended to prevent.

The arguments derived from a careful consideration of the mechanim

of the process of dilatation of the perineum and expulsion of head, all, in fact, lead us to the conclusion that the proper treatment of the perineum in labour is, under all ordinary circumstances, to abstain from active interference of any kind.

Is the conclusion arrived at by reasoning, countenanced by the results of practice?

In the works of Denman, we find the following:—"When women were delivered without assistance, I have not, in any case, observed any considerable laceration." (Vol. 1, page 60.) That the accident does occur, however, in cases where the delivery occurs unassisted is now well known. The explanation I have already given of this occurrence—that the laceration is due in these cases, not to the absence of assistance, but to the rapidity of the labour—I would now repeat.

Drs. McClintock and Hardy consider medical assistance so necessary, that they endeavour to explain why laceration does *not* occur in cases of unassisted labour, on the theory, "that inasmuch as these females are almost always involuntarily subjected to the deprivation we have mentioned, they habitually use their utmost endeavour to retard the birth of the child when they feel the head in vagina, in the hope of aid reaching them before the critical moment of delivery; and another reason is that such patients have been spared the ill effects arising from vaginal examinations." (Report, &c., p. 9.) It may, I think, be doubted whether patients do, as a rule, under such circumstances, exercise the kind of control mentioned. The exemption from laceration in such cases appears to be explicable in a very different manner. It is very certain that many deliveries, and rapid ones, do take place in the entire absence of all assistance, and with perfect safety to the mother, so far as the perineum is concerned. The only conclusion to be drawn from the fact is, that assistance must be of little service in ordinary cases, seeing that no bad result usually ensues in extraordinary ones when it is absent.

We cannot, at this moment, procure any considerable numerical testimony as to the results of the non-interfering system; the practice of supporting the perineum has been too universally adopted to allow of this being the case; but so far as the evidence procurable goes, it is calculated to give us every confidence in the resources of nature. Dr. West of Alford, and Dr. Swayne of Bristol, both assure us, from the results of a large number of cases, that the plan of "letting the perineum alone" has with them given very satisfactory results.

Is there, then, nothing to be done to preserve the perineum from laceration?

There are certain precautions which it is necessary to take, and to which I should be disposed to attach very considerable importance. When the head is passing through the vaginal outlet care must be taken that there be no impediment to its passage forwards. If the thighs of the patient be closed, and if the legs be in a straight line with the body, there may be an impediment of this kind, and the

natural result is, that the fourchette is called upon to bear a much greater strain than is necessary. The practice adopted in France, of delivering the woman lying on the back with the legs separated, is the one best calculated to remove this source of danger to the perineum. So, again, in the delivery of the shoulders, care must be taken to direct the delivery as much forwards as possible. It is very easy to see how infinite mischief may be produced by want of attention to this rule, and it is, in fact, extremely probable that laceration is not seldom due to this cause.

By adopting these simple precautions, I believe that the practitioner will do all that is possible to *prevent laceration* in ordinary cases.—*British Med. Journal, May* 11, 1861, *p.* 487.

102.—ON LACERATION OF THE PERINEUM.

By Dr. James Gray, Glasgow.

[Dr. Gray considers that Dr. Graily Hewitt has overlooked the main point in question; namely,—why laceration in natural labours takes place in one case and not in another. He takes it for granted that in every female the perineum is alike in its anatomical relations.]

I hold there is great diversity in its position, its length, and its relation to the pubis and coccyx. The pubic margin of the perineum or fourchette in one female is nearer the symphysis pubis than in another, while in a second it is nearer the coccyx; in a third, it holds an intermediate place, and so on; but I will class these diversities into three orders, which I shall designate—1. Coccygeal; 2. Axial; 3. Pubic.

In the first, or coccygeal, the labour progresses and terminates without rupture of the perineum, as the perineum is behind the axis of the outlet, and is not carried forward by the presenting part of the child's head as it emerges under the arch of the pubes.

In the second, or axial, the perineum holds a mid position, and, consequently, in such cases a slight rupture is apt to take place, as from the intermediate position of the fourchette, the presenting part of the head stretches and carries a portion of it forward.

In the third, or pubical, laceration can hardly be prevented; in this order the fourchette is anterior to the axis of the outlet, and as the head emerges under the pubic arch, it pushes the distended perineum forward funnel shaped, stretching it, in many cases, from three to five inches; as the bones do not give way, the head is more inclined to pass in the axis of the outlet.

I think this view of this subject sufficiently explains why rupture does not follow, in all cases, the birth of the child.

Dr. Hewitt says that supporting the perineum "must be rejected." I hold that by gently supporting the perineum in the second and third order, and guiding the head of the child forward in the direction

of the abdomen of the mother, in the great majority of cases, we may prevent laceration; at all events, it greatly lessens the rent.

Of 2896 cases attended personally, in only one did laceration occur and extend into the rectum. When this happened, the perineum was not supported; the labour was not rapid; neither was the head of the child very large; nor was there any deformity in the parts of the mother. I believe this accident might have been prevented.

As an auxiliary, chloroform ought not to be overlooked; for it is well known that under its influence the perineum becomes relaxed, and danger to that part is thereby very much lessened.

Conflict of opinion suggests an opposite line of practice, and a certain amount of truth may be on both sides. It would seem, therefore, to be practical to adopt a middle course without entirely rejecting either recommendation.—*British Med. Journal, June* 22, 1861, *p.* 654.

103.—ON SUPPORTING THE PERINEUM IN LABOUR.

By Dr. Edward W. Murphy, Professor of Midwifery in University College.

[Dr. Murphy gives us his views as follows:]

The whole action which takes place by which the womb is opened, the child forced through the pelvis, the perineum expanded and the head expelled, may be considered as a struggle of opposing forces. The tissues of the cervix uteri, the bones of the pelvis, the tissues of the perineum, are the resistance: the muscular power of the uterus, the overcoming force.

But such a struggle must not be considered merely as a question of dynamics; the action is vital, and every provision is made so to regulate it as to prevent injury. Thus, when the tissues of the cervix are to be dilated, the power of the uterus is kept under control by the liquor amnii; and if it escape by the suspension of its action, if the force of the pains excite irritation, every means are used to prevent the expulsive force of the uterus from suddenly expanding, or rather tearing through the tissues; nevertheless the cervix is frequently torn by the violence of the pains. So with regard to the perineum, if the membranes be entire, the most gradual and least irritating force expands it; if not, and the head descend, its pressure is only continued during the pain; when that ceases, the head retreats, and the irritation is removed.

Thus the expansion proceeds, the tissues becoming thinner and thinner; and although they are sometimes so attenuated that the head seems to press down almost into the world, yet it will again retreat until the next pain, the parts not being yet prepared to yield.

The practitioner is often disappointed by this delay, and uses every

effort to prevent a repetition of it; he therefore endeavours to direct the head forwards towards the pubes, and thus to force it over the perineum. I am not quite sure that this practice does not frequently cause slight laceration. But why does the head retreat when the perineum offers such little resistance? Because *the act of dilatation* has not taken place.

The tissues of the cervix uteri and the perineum both possess a power of expansion independent of the force employed against them. Both may be dilated suddenly and unexpectedly, and either may offer an obstinate resistance.

When this act of dilatation takes place, the head passes safely over the perineum; if it should not, there is always the risk, if not the certainty, that if the head be forced into the world, the perineum must be torn, and this whether it be thick and resisting, or thin and apparently yielding. Cases have occurred in which the tissues have been so expanded, that the head has been forced through the perineum, without the margin yielding in the least.

Hence in the management of the perineum, so as to save it from injury, the question arises—What prevents this act of dilatation taking place! The leading causes which interfere with it seem to me to be congestion and inflammation. If irritation be excited, congestion takes place, and the action is arrested; if inflammation take place, the perineum becomes still more unyielding. It remains, then, to determine whether these unfavourable results are more likely to occur when the perineum is supported by the hand of the practitioner, or if it be left to itself. There are very few cases in practice where the perineum is completely unsupported during the expulsion of the head, unless indeed in those instances of sudden expansion where the head is expelled by a single pain. It is impossible, therefore, to draw a comparison by quoting cases, but some conclusions can be formed by observing the symptoms which present themselves in an ordinary labour, when the perineum is being dilated.

No pains are so agonising as these; the support of the hand relieves the patient; she is conscious that her labour is assisted; at no period are the services of the practitioner more earnestly acknowledged. It is fair, therefore, to infer that this support relieves the irritation; so also it diminishes congestion, as pressure on distended vessels always does.

If such, then, be the case, the support of the perineum is essential, in order to prevent those causes coming into operation which interfere with the act of dilatation.

But besides this, the action of the uterus and that of the perineum do not always correspond; the one may resist the other, and the pains are sometimes so powerful as to tear through the perineum, even when most carefully supported.

What the result would be, when not assisted, may be decided by those cases where from some accidental cause the support is with-

drawn. One of the worst lacerations that ever came under my notice arose from the patient, an extremely excitable person, suddenly withdrawing herself during a violent pain; the perineum was deprived of its support, and was torn quite through. Every practitioner of experience can readily bring to his mind such cases.

Hence the support of the perineum is not alone for the purpose of allaying irritation and diminishing congestion, but also to counteract too violent distension from the action of the uterus. It is true that pressure so employed can never absolutely resist such powerful force, but it can control it, can moderate the power, and alter its direction.

Thus the pressure of the head forwards directs the force of the uterus towards the vulva and from the perineum; but in doing so, caution is necessary, lest the head be forced over the perineum before it is prepared. The rule to direct the head forwards is of value when the principle is understood, but most mischievous when it is not. It is one thing to assist in directing the force of the uterus from the perineum towards the vulva, but a perfectly different procedure to endeavour to force the head over a perineum not prepared to yield. This latter practice is, I fear, too frequently adopted, and causes many lacerations. Hence in my lectures I have stated that the object of supporting the perineum was to prevent its too violent distention by the action of the uterus, not for the purpose of forcing the head forwards.

The object of supporting the perineum seems to me to be twofold. First, to prevent or allay irritation and to diminish congestion, so that the act of dilatation may not be interfered with. Secondly, to counteract too violent action of the uterus.

The latter cause I have only referred to in my lectures, and the subject was left thus far incomplete; I have ventured, therefore, to trespass on your indulgence to allow me to supply the deficiency.

The only point on which I cannot agree with some of my colleagues on this practice, is the rule to press the head forwards over the perineum; I have omitted this because it is so constantly misunderstood and adopted as a means of concluding more rapidly a labour that may have been tedious. Forcing the head over a perineum unprepared is just as likely to cause its laceration as drawing it back, or leaving it to itself unsupported.—*British Med. Journal, April* 20, 1861, *p.* 426.

104.—ON RUPTURE OF THE PERINEUM DURING LABOUR.

By Dr. Thomas Skinner, Obstetric Physician to the Liverpool Dispensaries.

[In a case of severe rupture of the perineum, Dr. Skinner proceeded to remedy the laceration as follows:]

Within a few hours of the occurrence of the accident, and with the

kind assistance of Mr. Paterson, I placed the ruptured parts together again with two deep "needle sutures," and two superficial interrupted ones, all of which were removed on the morning of the fifth day, when Mr. Paterson and I had the satisfaction of seeing the rupture healed in its entirety; and so perfect was the line of union, that it was scarcely possible on the tenth or twelfth day to trace the *locale* of the lesion by the cicatrix.

I have little hesitation in stating that the "needle suture," as invented or reintroduced by my friend Dr. Aveling of Sheffield, is much less irritating, better borne by the patient, and is in every respect much to be preferred to the silk quilled suture. The thickness of the suture which I used was sixteen on the Parisian wire gauge; they are manufactured by Messrs. Cocker Brothers of Sheffield, and sold by Mr. Henry Aitken, cutler, of York, who supplied me. I think it proper to add that I bent the wire before introducing the needle point, that I passed each end through a smooth flat splint of wood, that I fixed them by means of pierced duck shot, and then removed the extremities. I would further add, that the sutures ought to be applied only sufficiently tight to bring the deep parts of the laceration into apposition, greater tightness will only do harm, by cutting tissue and causing congestion and irritation; it is quite simple, if necessary, to retighten the sutures as the subsidence of the congested state of the perineal structures takes place.

Remarks.—Denman has said, that when women were *delivered without assistance*, he had not in any case observed considerable laceration (5th edit. p. 79.) If this case teaches anything, it teaches most clearly that the whole tissues of the perineum may be ruptured by the unaided efforts of nature, without even *voluntary* attempts at bearing down on the part of the mother. I am told by the patient that she tried rather to restrain herself than otherwise. The mechanism of the rupture in this case seems to have been too great rapidity of the second stage; the perineum having had no time to be safely dilated; and the fact of the fœtal head being rather within the average size, twelve inches in circumference, the average circumference being fourteen inches. This case also shows that very severe ruptures of the perineum are much better repaired at the time than left to themselves or to the alternative of rawing the edges on a future occasion, not to speak of the chances and evils of rectocele and prolapsus uteri in the interval.

I would further observe, that so far as the *minor* lacerations of the perineum are concerned, I am not disposed to believe in the unaided restorative powers of nature. I have frequently looked for it, but in vain. Contraction of the swollen and dilated parts, and of the edges of the wound, undoubtedly do take place; but coaptation and adhesion of the lacerated structures I have rarely or never seen to my satisfaction. So thoroughly am I convinced of this, that, for several years back, I have never trusted to nature alone, but have always intro-

duced a twisted or quilled suture, or two, as the case may have required. I can safely say that I have never regretted doing so; on the contrary, I have always been extremely satisfied with the result; and I am strongly of opinion that, if an examination of the condition of the perineum was instituted, *post partum*, as frequently as the same structure is vainly supported during labour, and treated as I have stated, my friends Dr. Keiller of Edinburgh and Mr. Baker Brown of London would have fewer operations to record for rectocele and prolapsus uteri. When we consider that an incalculable amount of the future physical comfort of the patient, and no small portion of the pleasure and happiness of matrimonial life, depend upon the strict integrity of the structures of the perineum, I really think that we are guilty of culpable neglect, or of ignorance, to say the least, in leaving the rupture to nature, or, what is very much the same thing, *leaving it unobserved.—Brit. Med Journal, Aug.* 17, 1861, *p.* 170.

105.—IS THE ERGOT OF RYE, WHEN ADMINISTERED TO THE MOTHER DURING LABOUR. DANGEROUS OR NOT TO THE LIFE OF THE CHILD?

By Dr. Uvedale West, Vice-President of the Obstetrical Society of London.

In Dec. 1855, the author published the particulars and results of an inquiry similar to the present—to the effect that, in a series of 69 cases in which he had administered the ergot of rye on a gross total of 278 labours, 9 children were still-born—viz., 2 putrid at birth; 2 born after labours which were preceded and accompanied by hemorrhage; 1 born footling, with hydrocephalic head, and consequent fatal compression of funis; 1 in which there was evidence of latent compression of funis; 1 a difficult primiparous forceps delivery; 1 a difficult vectis delivery, the mother seriously ill from excessive œdema; and 1 born dead without any assignable cause.

In consequence of certain criticisms on this paper which were published in France, accounting variously for the results, and challenging the author to continue the inquiry on the same plan, one of the critics, Dr. Danyau, on the part of the Imperial Academy of Medicine, maintaining that, unfavourable though the results appeared to be to him, yet a similar report on a like number of cases would probably be more unfavourable still, Dr. West continued to tabulate his ergot cases as he had begun, with the results which he now begged to lay before the Society:—

Between Dec. 23rd, 1855, and June 22nd in the present year, on a gross total of 734 labours attended by the author, the ergot was given in 172 cases, including one case of twins, so that there were 173 children born under the effects of ergot of rye. Of these, only 5 were still-born from all causes—viz., 3 putrid at birth; 1 with pla-

centa prævia and profuse hemorrhage—premature; and 1 with prolapsed funis, detected an hour after the ergot was given, the operation of turning, which was then immediately performed, having been too late to save the child's life. The author urged that, in reply to Dr. Danyau's precise challenge, he might fairly have rested contented with the results of the first 69 cases of this second series, in which there was not a single still-birth from any cause; but he considered that that fact, when compared with the unfavourable result of 9 still-births in the preceding series of 69 ergot cases, proved only that 69 was too small a number to form a correct conclusion from, and therefore he went in with the inquiry until it was spread over a gross total of 1013 labours, on which number the ergot was given in 241 cases. Of that number of 241 ergot cases, including 242 children born, there were, adding the 9 of the first series to the 5 of the second, 14 still-births from all causes—viz., 5 putrid at birth; 1 footling; 1 prolapsed funis; 3 hemorrhage during labour from placenta prævia; 2 difficult instrumental deliveries; 1 suspected latent compression of funis; 1, cause not manifest. On the whole number of births—1013 labours, and 1029 children born, including the series without ergot as well as that with ergot—there were 50 still births; of which 5 were born dead without any manifest cause. So that, inasmuch as only 1 of those 5 was born under the influence of ergot, that medium having been given in the greater proportion of 1 in $4\frac{3}{4}$ of the whole number of labours, the author thought there was no sufficient evidence in the facts he had accumulated to justify the doctrine that ergot of rye was dangerous to the life of the child.

As to the mother, the author found the following results on a retrospect of the gross number of 1013 labours—viz., 7 deaths within the lying-in month, of which 1 occurred with a patient who had had ergot of rye; 18 cases of incarcerated placenta, of which 5 were after ergot of rye; 25 instances of post-partum hemorrhage, of which 5 were after ergot of rye; 30 cases of puerperal disease or disorder, of which 9 were after ergot of rye. So that he was led to the conclusion that ergot has little or no influence in either causing or preventing parturient or puerperal accidents or diseases. At the same time he wished to remark, that probably in proportion as ergot might improve uterine action, certain accidents which depended on deficient uterine action, such as post-partum hemorrhage and after-pains, might be controlled by ergot of rye.

The author concluded his paper with the observation that the tables before the society appeared to prove that it was immaterial in what stage of the labour the medicine was given; whether the os uteri was rigid or supple; whether the liquor amnii was or was not evacuated; or whether the mother was multiparous or primiparous; but that it was essential that actual labour should be present, as well as that the accoucheur should be competent to meet any emergency that might arise, just the same as when ergot has not been given;

that ergot was useful wherever it was desirable to improve uterine action; and that it could be dangerous only where uterine action would be dangerous, as, for example, in a case of arm presentation after the liquor amnii was evacuated, as then it would make turning more difficult.—*Lancet, July* 20, 1861, *p.* 62.

106.—ON THE USE OF THE FORCEPS IN TEDIOUS LABOUR.

By Dr. G. Hamilton, Falkirk.

[The use of the forceps in labour is on the increase. If skilfully used, and at the proper time, there is no doubt a great deal of time and suffering are saved, with comparative safety to both mother and child. In the early stage of labour, before the dilatation of the os uteri, we can do nothing but wait patiently.]

When, however, the os uteri has become dilated, and the head has entered the pelvis, so that an ear can be easily felt, I hold that the danger to the child usually becomes imminent if it be allowed to remain undelivered much more than two hours, especially where the pains are smart, or the compression is considerable. When mother and child have been much exhausted by a protracted or very severe labour, my rule is, when an ear comes within reach, not to wait more than a quarter or half an hour, or even, if the case is urgent, to deliver immediately. I always feel anxious in such instances as I have last mentioned to get quickly over the latter half of labour; for though no serious harm may yet have happened to mother or child, there can be no question that, to the latter at least, much additional delay would be fatal. If, therefore, nature is getting over this portion of the labour quickly, I am content; if not, I use the forceps, and deliver.

It is always an essential preliminary in applying the forceps to ascertain exactly the position of the head, but practitioners, I think, will allow that it is sometimes not easy at once to settle this satisfactorily. If the tip of an ear can be felt, and I have still any doubt on the point, I fix the forceps over the ears and exert only traction, until the head has somewhat advanced, when I remove the instruments and satisfy myself of the exact position. I have said in my former papers that when this has been done it is most important, in a large proportion of the cases, gently to turn the head round at the same time that traction is being applied, so as to bring the face into the hollow of the sacrum. Sometimes this can be best done by making it turn towards the right side, while in other instances the left will be found the easier. Nature, if left to herself, usually, I imagine, takes the shortest course. When the practitioner interferes, perhaps the invariable rule ought to be at first to endeavour to turn to the right side, when the face is towards that side, opposite the symphisis

pubis, or even slightly to its left, as the rectum on the left side diminishes the capacity of the pelvis, and offers an obstacle to the free version of the head. When the face is opposite the left acetabulum, or the left sacro-iliac synchondrosis, I usually try first to turn by the left into the hollow of the sacrum; but, should this fail, I think that the accoucheur has not exhausted the resources of his art until he has endeavoured to turn the face round towards the *right* side, for delivery will sometimes be found practicable by this circuitous course when the direct one by the left is impassable. I suspect that this position, with the face to the left, from the obstruction referred to, is one that accoucheurs have often found among the most formidable they have had to encounter. Mere traction is of little avail, for nature, as I have remarked, will take the short route, and the obstacles there may be insurmountable. It is only within these few years, I confess, that I have become fully impressed with the importance of this line of practice, and the extent to which version of the head can be carried. As the circumstances connected with this are very instructive, they will be, I hope, a good apology for my giving a few details.

I was requested, at 8 p.m., in July 1857, by a brother practitioner, to see, along with him, a primipara in labour. I found that the woman had been in labour about eighteen hours, and that, five hours previously, my friend had attempted to deliver with the forceps, but had been unable to do so. He therefore wished me to apply them. Upon examination, I found that the vertex presented, and that an ear could with difficulty be felt near the right acetabulum, with the face towards the left side. I had no difficulty in applying the forceps over the ears, and succeeded after a while in advancing the head a little, at the same time that it was easily made to rotate so as to move the ear felt from the right side to opposite the left acetabulum. Here the opposition of the rectum was encountered, so that though, while the forceps were being used, the face felt constantly on the point of passing into the hollow of the sacrum, whenever these were removed, the ear had a tendency to come again nearly opposite the symphisis, the head of course assuming a nearly transverse position. As the most persevering efforts with the forceps, as well as excellent pains, had failed at 6 a.m. next morning materially to advance the head, it became imperative, for the sake of the mother, that delivery should be effected. Accordingly, the head was opened, and delivery accomplished without any delay, the mother afterwards making a rather lingering recovery.

Two circumstances struck me in this case,—first, the ease with which the head could be made to rotate, so as to bring the face opposite the left sacro-iliac synchondrosis, and the yet impossibility of carrying it into the hollow of the sacrum, or of bringing it nearer the outlet in any material degree; and, secondly, the very small quantity of brain which required to be abstracted in order to make the passage

of the head quite easy. Reflecting upon these circumstances, I had a strong impression that, had the face in this instance been towards the right instead of the left side, or could it have been turned to that side, the child might have been saved.

On Sept. 20th following, a case very similar in position and circumstances to the one mentioned occurred to me. The woman had previously had four children, all of whom had been delivered with the forceps —the two last by myself, without any great difficulty. The vertex presented, with an ear opposite the symphisis pubis and the face to the left side. The head having continued in this position, without making a sufficient advance, as long as I thought it prudent to allow it, I applied the forceps over the ears, and again easily caused the head to advance slightly, and the face to come opposite the left synchondrosis, but again also I found that no further progress could be effected. Instead, therefore, of persisting in attempts to force the face to pass over the rectum, I resolved to try whether I could make it sweep round the pelvis, so as to bring it opposite the *right* synchondrosis. With the aid of the forceps first, and afterwards of my hand, I found no great difficulty in pushing up and rotating the head, so as in a short time to effect this. Having thus got rid of the opposing rectum, the forceps were again applied, and the child safely delivered without much difficulty.

This operation will of course generally be easier when the face is towards the symphisis pubis, but that this is not invariably the following case will show. It was my 732nd case, and broke the long continuity of successful deliveries, and I therefore give its details the more willingly, in order to show the formidable difficulties which had to be encountered. The patient was a farmer's wife, with a remarkably hard muscular development, who on two former occasions had had abortions about the fifth month. I saw her about six a.m., and for some hours the labour went on slowly but favourably. In the course of the forenoon, however, she was suddenly seized with a convulsive fit, not very severe, but which still somewhat affected her consciousness when she came out of it. The convulsions continued to recur every hour or so until she was delivered about 3 p.m. As she was tolerably easy during the intervals, I allowed the labour at first to proceed without attempting to interfere, the advance then made being such as would have rendered the application of the forceps very difficult. Afterwards, when the os uteri had become perfectly dilated, and an ear could be just felt, I applied them, and succeeded, after considerable exertion, in bringing the head so far within reach as to enable me to satisfy myself that the face was nearly opposite the symphisis pubis. Simple traction I now found did no good. I therefore endeavoured to turn the face to the right; but, after persevering efforts both with hand and forceps, I could produce no good result. As a last resource with the forceps, I then tried version to the left, using traction along with it, and, to my surprise and

delight found that the face slipped without much difficulty into the hollow of the sacrum. The delivery was then easily effected. The child, however, was still-born. The mother's recovery was remarkably favourable, considering the severity of the labour, but a purulent vaginal discharge continued for some months after the labour. This, I may remark, is the only instance in all the series of cases I have referred to in which I am aware of such an occurrence having taken place, or where, indeed, any local injury of the slightest consequence was sustained.

It is always desirable that the head should have emerged from the uterus before the forceps are applied, and determined efforts should be made with the fingers or hand to effect this. Nevertheless, imminent danger to child or mother, or to both, sometimes requires that the instruments should be used before the head has passed the os uteri. The last case was an example of this kind, when I first applied the forceps. My impression is, after frequently having had to use them under these circumstances, that where caution is employed, the risk is not great. The only case, indeed, I have ever had which could make me doubt of its safety, was one which occurred to me very lately. In a primipara, when called, I found the os uteri well dilated, and about a foot of the funis prolapsed, the arteries, however, beating strongly. I tried to replace it, but not being able to do so, I immediately applied the forceps, and delivered with great ease, and so quickly as to enable the child to survive. Mother and child apparently did well for several days, and I had ceased seeing them, when I was sent for to the mother, who had been seized with diarrhœa. Though this was controlled, a remarkable degree of prostration accompanied it, some abdominal tenderness came on, and she sank on the tenth day after delivery. Although I have thought it right to mention this case, as puerperal fever was occurring about the same time in the district, it seems to me impossible to say whether, or how far, the instrumental interference was connected with the fatal result.—*Edinburgh Medical Journal, Oct.* 1861, *p.* 315.

107.—ON A MODIFIED FORM OF OBSTETRIC FORCEPS, TO BE USED IN CASES WHERE THE ORDINARY ONES ARE INAPPLICABLE, OWING TO UNUSUAL ELONGATION OF THE FŒTAL HEAD.

By Dr. Graily Hewitt, Physician to the British Lying-In Hospital, &c.

[When the fœtal head is large, and the passages not over wide and dilatable, the labour is much prolonged, and the head of the fœtus becomes excessively elongated. The ordinary obstetric forceps are not applicable to such a form of head, or only with great difficulty, the blades being too short and too sharply curved to embrace the head.]

The occipito-mental diameter of the head, with which we are chiefly concerned in reference to the applicability of the forceps, measures, according to various obstetric authorities, from $4\frac{1}{2}$ in. to $5\frac{1}{2}$ in. Van Pelt, who has recently published the results of his measurements in 700 cases, found the average to be $5\frac{17}{40}$ in. These measurements were made, not immediately after the escape of the head, but when sufficient opportunity had been given to the head to recover from the effects of compression during the parturitive act. No observations had hitherto been made for the purpose of ascertaining the difference between the measurement of the occipito-mental diameter during parturition and at a period a few hours after the head had been born. The difference in question was known, however, practically to be considerable.

The relations subsisting between the curve and length of the blades in the ordinary forceps and the shape and curve of the head in ordinary cases and in cases of undue elongation were next alluded to, and illustrated by means of drawings. Thus it was made evident—1. That the ovoid presented by the head (as it is in ordinary cases) is such that the ordinary forceps, with a length of blade of from $6\frac{1}{2}$ in. to 7 in., and a curve the arc of a circle of from 10 in. to 11 in., is admirably adapted to it. 2. That when the ovoid exceeds $5\frac{1}{2}$ in. in its long diameter, the forceps in question is likely to be inapplicable, and, as in the case alluded to, difficulty might arise in endeavouring to apply it. How often would such exceptional elongation be observed in practice? In Van Pelt's cases there were 28 out of 646 in which, some little time *after* the end of the labour, the long diameter measured $5\frac{3}{4}$ in. Hence in about 3 per cent. of all cases we may expect to find the head so long as to give rise to the difficulty in question, and this not taking into consideration cases in which there may be undue elongation, and a less degree of difficulty in cases where the head is of medium size, but yet unusually elongated. The author believed that the difficulty alluded to had been (how frequently it was impossible to say) encountered in practice by others, though not recognised. To those who would object that, admitting the possibility of the difficulty occurring, it must yet occur very rarely, he would reply that, admitting the rarity of the occurrence in question, it was extremely desirable that it should be recognised, and that this or any other misconception likely to arise in connexion with the use of so important an instrument as the forceps should be removed. The only modification in the forceps hitherto admitted or insisted upon had reference to the *position* of the head in the parturient passage. No modification having reference to the *shape* of the head had as yet been insisted on. The latter principle was the one he wished to see adopted. The author exhibited to the society an instrument constructed under his directions, the blade of which was 8 in. long (instead of $6\frac{1}{2}$ in. or 7 in.), the curve that of a circle of 14 in. (instead

of 10 in. or 11 in.), and identical in other respects with the ordinary short forceps used in this country. The curve more nearly approached that of the French instrument. The instrument thus modified would, he believed, be found applicable in the class of cases now under consideration—those in which the fœtal head is unduly lengthened. The cases requiring it would be easily recognised ; the blade of the forceps used as a probe would inform the operator whether the shape of the head required one or the other instrument. The ordinary instrument used in this country was perfectly adapted to all ordinary cases, and it was only a further development of the principle on which that instrument was constructed to insist on the necessity for occasionally employing a forceps of which the blades were by their length and shape suited to the altered length and shape of the fœtal head. In the long, tedious labours of primiparæ the new instrument would be most likely to be called into requisition.

[In the discussion which followed the reading of Dr. Hewitt's paper before the Obstetrical Society of London]

Dr. Barnes said he had encountered the practical difficulty signalized by Dr. Graily Hewitt, but had not met it in the same way. At one time he was in the habit of using the Dublin straight forceps, which is somewhat longer than the one described by Dr. Hewitt as being in ordinary use in England. This instrument he had now abandoned ; it was apt to split the perineum. This objection he thought applied to all straight short forceps. To introduce this instrument, it was necessary to press the handles far back, stretching the perineum backwards, and then during extraction, as the bow of the blades sprang directly from the handles, a great strain upon the perineum was unavoidable. For some years past he had used exclusively the forceps of Mr. Roberton, of Manchester. Being called upon frequently to apply the forceps, he was able to say that this instrument was admirably adapted to all the ordinary emergencies, whether the head lay at the brim, in the cavity, or near the outlet. The pelvic curve, the length of the blades, and the length of the shank, kept the perineum free from any strain whatever beyond what was incidental to the passage of the head itself. The instrument added nothing to the danger to the soft parts. He wished to add, that it was not a fair representation of the application of the forceps to show it as embracing the head with perfect symmetry. The grasp was oblique, one blade lying on the right brow or temple, the other on the occiput or behind the line of the transverse diameter. This was the necessary consequence of the circumstance that the long forceps was introduced in the transverse diameter of the pelvis, and not with reference to the child's head. This he thought an important rule to follow. In some cases, with the head low in the pelvis, it might be possible to apply the short forceps with a blade on either parietal bone ; but it was a bad practice. The pelvis should be the guide for the position of the forceps, not the head.

[Dr. Braxton Hicks observed that the short straight forceps could not be passed forward enough to lock, and by employing the long curved forceps this difficulty was overcome.]—*Lancet, June* 15, 1861, *p.* 590.

108.—SOME PRACTICAL OBSERVATIONS ON THE TIMELY USE OF THE OBSTETRIC FORCEPS.

By Dr. Edward B. Sinclair, A.M., Fellow and Censor of the King and Queen's College of Physicians in Ireland; Ex-Assistant Physician to the Dublin Lying-in Hospital, &c.

It has been said by a high authority, that "in every case of midwifery, the chief object to be attained by the practitioner should be, the preservation of the *lives* of both mother and child committed to his care;" and this has formed the basis of treatment in all cases of labour. Now, though I thoroughly concur in the above rule, yet I do not consider it without fault; it does not go far enough, in my opinion. I conceive its great author would have been more judicious, had he defined the chief object of the obstetric practitioner to be, the preservation of *the structures of both mother and child, and the prevention of fever and inflammation* during and after parturition. Acting on this dictum it would be found, that the lives committed to his charge, especially that of the mother, would seldom be placed in jeopardy. The former dictum has given rise to what may be called the expectant line of practice in obstetrics, which, however judiciously employed, has, generally speaking, been carried too far. Too great a reliance on the powers of nature, albeit the lives of mother and child may have been preserved *at the time*, has often given rise to occurrences which have rendered the post-partum state not only dangerous and protracted, but fatal; or what is as bad, has caused the life of the woman, so saved, to be a burden to her. Therefore let us look, not to the preservation of life merely during parturition, when conducting a labour, but to the conservation of the tissues, and the prevention of inflammation. Then life will follow as a matter of course, and that unclouded by any permanent defect.

For many years a portion of the Dublin school of midwifery, in conjunction with some members of that of England, and also of Scotland, have manfully struggled against, what I fear I must designate, the narrow prejudice of some of the heads of the profession, in endeavouring to establish a more rational practice with respect to the application of the forceps. In our school I could mention a goodly list of gladiators, and amongst them would stand conspicuous those of the elder Beatty, his son, the present eminent obstetric practitioner, Evory Kennedy, Shekleton, and Churchill; several equally eminent gentlemen from the sister countries I could also enumerate, did space permit. But it is no matter of surprise to me that practitioners should have

been guilty of errors of omission with regard to the application of this instrument, when I review the rules laid down for their guidance. Some of these I shall take the liberty of quoting; premising, that so highly did their authors stand in the estimation of the profession, that their word was considered almost a law. And it must be also observed that their dicta, as will appear, were uttered with the greatest solemnity and precision; yet, notwithstanding, they are frequently found with equal solemnity contradicting themselves.

Dr. Collins, one of the former Masters of the Dublin Lying-in Hospital, thus writes:—"Let it be carefully recollected that, so long as the head advances, *ever so slowly*, the patient's pulse continues good, the abdomen free from pain on pressure, and there is no obstruction to the removal of the urine, interference" (that is, of any kind) "should not be attempted, unless the child be dead." Now, from this we must infer that the state which is the converse of the above, viz., where amongst other matters, the symptoms of inflammation, indicated by quick pulse and tenderness on pressure, *are* present, we should use the forceps. But, in all likelihood, the life of the child shall then, indeed, be found extinct; so that this rule would ignore the forceps altogether, and turn cases which might have terminated happily into craniotomies. Again, prior to interference with the forceps, "the head of the child shall have rested for six hours as low as the perineum, though the pains have already ceased." Therefore, if the pains continue so that the presentation advances ever so slowly, we should not use the forceps; and though the pains or uterine action may have ceased for more than five hours, the head making no advance whatever, the practitioner is not to interfere with this instrument.

Dr. Murphy, late Assistant-Physician to the Dublin Lying-in Hospital, and at present Professor of Obstetric Medicine in the University of London, gives very nearly the same advice. "It appears to me," he says, "that four hours would be quite sufficient to allow the head to remain in the same position to authorize your interference." These two last recommendations show that the eminent gentlemen who put them forward considered the question merely one of time, apparently forgetting that the powers of endurance are not equal with all women—that some could bear with impunity, for many hours, what would kill others to endure for one. How uncertain, how unsafe, then, are such precepts!

The rules of practice above quoted evidently originated from the axiom of Denman (who may justly be called the father of British obstetric medicine), as follows:—"It is meant, when the forceps are used, to supply with them the insufficiency or want of labour pains; but *so long as the pains continue*, we have *reason to hope* they will produce their effect, *and shall be justified in waiting*."

Again, it has been taught that "when the pulse becomes quick, the tongue dry, the discharges foul, and the mother's parts hot and swollen," the time has arrived for the application of the forceps. In

other words, this teaching directs that the forceps, which I assert should be used for the purpose of *preventing* the occurrence of inflammation, ought not to be had recourse to till *that inflammation has set in.* Dr. Ramsbotham's teaching goes far beyond this, and tends to ignore altogether the effectual application of the forceps. He tells us, "if the pains are subsiding gradually, or have entirely disappeared; if the strength is failing, the spirit sinking, the countenance becoming anxious; if the pulse be 120, 130, 140, in the minute; the tongue coated with white slime, or dry, brown, and ropy; if there have been two or three rigors; if, on pressing the abdomen, there is *great* tenderness of the uterus; if there be green discharge; if there be preternatural soreness of the vulva, with heat and tumefaction of the vagina; if the *head have been locked for four hours, and made no progress for six or eight hours: if the patient be vomiting a dark, coffee-ground-like matter; if there be hurried breathing, delirium, or coldness of the extremities, then* we are warranted in having recourse to the forceps, although the labour has not lasted the limited period of twenty-four hours, or even twelve:" he adds, however, "and we should be acting injudiciously(!) to allow the case to proceed until the last four symptoms appear without relief being afforded."

Omitting, then, the last four symptoms, it seems to me, as I dare say it has appeared to many a junior practitioner, that, according to Dr. Ramsbotham, not until the parturient woman exhibits to her attendant impending dissolution, should he use the forceps; but that he is not to wait for the occurrence of the last four symptoms, indicating that she is absolutely in *articulo mortis.* Still more adding to the perplexity of those who may consult this author for information as to the period when the forceps is applicable, we find him, in another place, telling us emphatically to use it without loss of time; for, says he, "*bear in mind* that more injury may accrue from too long delay than arises from premature assistance!" Strange doctrine after his prior advice! I could multiply quotations, did space permit, tending to show the uncertainty of the teaching of authors on this question; and the perplexity and fear that must, consequently, have arisen amongst the mass of practitioners, concerning the operation of delivery with the forceps. Suffice it to add one more.

Dr. William Osborne, in his 'Essays,' informs us—"All the powers of life are exhausted, all capacity for further exertion at an end, and the mind as much depressed as the body; they would at length sink together under the influence of such continued but unavailing struggles, unless rescued from it by means of art:"—that means being the forceps. How truly does Dr. Burns say "that the disciples of the school of patience carry their fears of the mischief resulting from the use of the forceps to an extravagant length, and place a mistaken confidence in the efficacy and safety of a continued action of the expulsive powers;" and how correct is the observation of our latest authority on this operation, Dr. Churchill, who writes as follows:—

"It must be admitted that, until lately, we have been too much afraid of the forceps, and have allowed cases to become subjects of craniotomy, which, at an earlier period, might have been safely delivered with the forceps."

It would, indeed, appear that until very lately the general tendency of obstetric teachers, especially in this city, was not to teach the use of the forceps, but its abuse. With few exceptions, they shrouded this instrument with such suspicion and horror, that practitioners were deterred from availing themselves of it. The difficulties and dangers attending the use of the instrument were ever brought prominently forward, to an exaggerated degree; while the facility of its application and the safety of its action were kept completely in the back-ground. I have no doubt, entertaining as I do the highest respect for the eminent physicians, that those who thus taught in this city were actuated by the purest motives—that they thus taught with a laudable desire of preventing as much as possible the origin of a system of "meddlesome midwifery." But it is much to be feared, that, running to extremes with their views, a great loss of life and comfort was the result; not so evident, however, in private practice, as in hospitals and charitable lying-in institutions, where the physician meets a greater amount of ill-health and general constitutional delicacy. The rule till recently was, to postpone the operation as long as possible—not to act till those symptoms occurred, the advent of which should have been rendered impossible. Life, certainly, was often preserved; but the operation was performed too late to hinder subsequent inflammatory action of the mother's tissues, and all the dangers attendant thereon. I well remember listening to Dr. Churchill's eminent predecessor, picturing in his usual powerful language the dangers and difficulties of forceps delivery. So terrible a picture did he pourtray, that just after having heard him lecture on the subject, I would rather have attempted to tie the abdominal aorta than use the midwifery forceps. But a short time since all lacerations, fistulæ, and cicatrices were laid at the door of instruments; while the fact of the head having continued to press for a long time in one fixed situation was never taken into account. It was thus that, when a vesico-vaginal fistula arrived in this city from the country, we have so often heard the following story:—The patient was a long time in labour; a dispensary doctor was called in, and he used the forceps. Well, here was a forceps delivery, and the woman has got a vesico-vaginal fistula; so the forceps caused the inflammation, which resulted in sloughing and fistula. But the long labour was not taken into consideration. That the results of inflammation from prolonged pressure of the head in one position is sometimes unjustly attributed to the use of instruments, I shall mention the following circumstance to illustrate; and though this anecdote relates to the perforator, still, on consideration of the facts, it will be found more applicable than if it refered to the forceps.

A little better than two years since, I was requested by the medical attendant of a large public institution to visit a woman there in labour. Upon inquiry, I found she had suffered strong uterine action for more than thirty hours. When I saw her, the tongue was dry and brown, the pulse quick: there was much thirst: the parts were hot, swollen, and tender; a fetid olive-coloured discharge flowed from the vulva; the perineum was partially distended by a large scalp tumour, and was absolutely at the moment in a state of inflammation. The head of the child was completely impacted—not even a flat catheter could be introduced into the bladder (which fortunately was not very full), neither was there room to have insinuated its beak at any point between the fœtal head and the pelvis. The fœtal heart could not be heard. Of course I perforated at once, and as soon as I had evacuated the contents of the cranium as completely as possible with the crotchet, I removed that instrument, and by means of my fingers hooked through the cranial opening, with the utmost possible ease I completed the delivery, which did not take twenty minutes from first to last. I left directions, amongst others, for treating the inflamed parts, and put the nurse on the *qui vive* for sloughing. The gentleman for whom I acted took up the case, and the next day I heard the poor woman was going on as well as could be expected. I may here remark, she ultimately recovered, without any serious inconvenience. Some days, however, after this operation, I met a gentleman who occupies an important post in one of our large medical hospitals; he laughingly informed me, in a manner not to be mistaken, that he had seen a patient of mine with "*sloughing after instrumental delivery.*" This gentleman had been asked, by the one for whom I had previously acted, to take his duty for a day or two, soon after I had operated; saw this midwifery case amongst others; heard that I had used instruments—and at once connected the sloughing with the instruments; never considering that even *though instruments had been used, the sloughing might have been caused by long pressure of the head.* I do not think I ever convinced him that I was not to blame for the results. Yet here *no instrument had touched the mother's parts;* and the head after it had been lessened, passed through them so readily as to require the hooked finger only to extract it. I could relate many such anecdotes as this. Now, if persons would only consider the parts of the mother which generally come in contact with the forceps during delivery—if they recollected that the blades were adjusted in an obliquely lateral position; that they were more or less transverse, as regards the pelvic space of the mother; that they are very seldom put on antero-posteriorly, the only position in which they could exercise direct pressure on the urethra; could they be convinced that, when adjusted in this latter position, the site of the urethra would correspond to the fenestrum of the pubic blade; that the only portion of that blade which could ever come in contact with the urethral portion of the vagina, was the beak, and that this beak pressed it only

during the application of the instrument and during extraction of the head: that its pressure was very transitory, and that this transitory pressure was exerted *all along the course of the urethra, and not at one point more than another*—they would not, I can scarcely think, charge the forceps so often with the crime of vesico-vaginal fistula. I firmly maintain, that in careful hands, no injury can be inflicted by this instrument either upon mother or child. But the subjects concerning which I wish chiefly to comment, in connexion with this instrument, are the post-partum conditions of the lying-in woman.

Firstly, then, I contend that the early application of the forceps prevents inflammation of the mother's tissues, and its results.

Secondly, that the free and timely use of this instrument is a powerful means for warding off disease and death, especially in public obstetric institutions.

With respect to the first assertion, I regret it is not in my power to offer any statistics relative to sloughing of the vagina from authors who are hostile to the free use of the forceps; and it is a curious fact that some of these very authors *have* published obstetric statistics, and yet are silent upon this subject; they give no account of their cases of sloughing. I can, however, produce the statistics of a gentleman upon this point, who at one period used the forceps most freely. And premising that the majority of the cases of sloughing occurred during a period when this physician was still prejudiced by old and respected opinions, I think it will be considered, when I have exhibited the figures, that not only is the forceps guiltless of the charges laid against it, but also that there was an extraordinary immunity from sloughing of the soft parts of the mother, during Dr. Shekleton's mastership of the Dublin Lying-in Hospital, and that in consequence of the free use of this instrument. I can answer for the truth of the statistics I produce; they have been published by myself and Dr. George Johnson, in our report of Dr. Shekleton's mastership, all the facts having been registered at the moment of their occurrence.

Out of 13,748 deliveries of every description (225 of which were delivered with the forceps), there occurred but twenty cases of sloughing (more or less) of the soft parts; seven of these happened in deformed pelves after perforation and crotchet delivery; five occurred in tedious labours, where no instruments had been used; two in prolapse of funis, and one in retained placenta; in neither of which the forceps was used, but they were subsequently seized with erysipelas, sloughing, and died—or, fifteen cases of sloughing, in which the forceps had not been used; leaving five cases only which occurred after forceps delivery—three of these latter were in single births, out of 200 such forceps cases, all slight, and after long labour: these recovered. There were two instances in twin-forceps-labours, in both after long delay, and one of these died.

Out of 13,748 deliveries, there were only four examples of urethro-vaginal fistula after convalescence, two of which completely closed;

one contracted to a minute opening; and in one instance the woman died, subsequently, of peritonitis.

But had the forceps been used more freely, and much more early, in the first periods of Dr. Shekleton's mastership, I am almost certain that the five cases which occurred in tedious labour, when instruments had not been used, and the five which occurred under the head of forceps deliveries, would not have been recorded.

That the free and timely use of the forceps is a powerful means of warding off disease and death, especially in public obstetric institutions, I think I can produce strong reasons for maintaining.

Burns, when speaking of the effect of delay in labour, says, "There is a strong disposition given to puerperal diseases, not merely to those troublesome though less dangerous complaints known under the name of weids or irregular febrile paroxysms, but also to more formidable affections of an inflammatory nature, especially of the womb, and peritonitis; accordingly we find that a much larger proportion of women die after protracted than after natural labour." This has been corroborated in an elaborate dissertation by Dr. Simpson, of Edinburgh; and I believe no one will have the hardihood to contradict it. Lessen, then, the number of tedious cases, and it follows that the practitioner shall have a cleaner bill of health, and the minimum of mortality.

To illustrate this, I shall again consult the statistics of the Dublin Lying-in Hospital; and for the purpose of more fully proving my position, I shall divide Dr. Shekleton's mastership into three periods, namely, the 1st, during the months of Nov. and Dec., 1847; 2nd, during the years 1848 and 1849; and 3rd, from Jan., 1850, to Nov. 1854. I have made this division, because during the first period the forceps was not used at all; during the second, prejudice was merely commencing to yield to common sense; and at the commencement of the five last years, the necessity of the early application of the forceps was fully appreciated.

Now, comparing the number of deaths with that of the deliveries and the ratio of forceps cases during each period, I find the following to be the results:—

During November and December, 1847, there were 242 deliveries, and as many as 17 deaths (to say nothing of cases of disease), that is, one in about every 14 women delivered died; there was no forceps delivery. During 1848–49 there were 3,886 deliveries, and but 70 deaths from all causes, or one in about 55 of those delivered; and the forceps, during this period, was used 38 times, or once in every 108th case. And during the years 1850 to November, 1854, there occurred 9,620 deliveries, of which but 78 died from all causes, or one in every 123, nearly 124; and the forceps was used 187 times, or once in about every 50th case. And were those deaths taken into account only which had originated from puerperal causes, the absolute puerperal mortality would have been one in about 350 of those delivered.

Comparing the ratio of disease with that of forceps deliveries, and confining ourselves merely to such seizures as may come under the term puerperal fever, I find the following to be the results during these three epochs :—

During the two months of 1847 puerperal fever was rife, Dr. Shekleton having received charge of the hospital during an epidemic, and all the cases of illness might have been included under that head. This state of the house first induced Dr. Shekleton to lessen his second stages, and was the means of bringing forward the forceps. But during the years 1848–49, when the forceps was used once in every 108 cases, puerperal fever and its allies were in the ratio of one to every 54 of those delivered, the numbers being 72 cases of puerperal fever out of 3.886 deliveries ; whereas, during the last five years, when there were 9,620 deliveries, and the forceps was used once in every 50th case, puerperal fever and its allied affections were as low as one in every 104 of those confined, the examples of these affections amounting only to 47.

As I have spoken of the uncertainty of the rules laid down for the guidance of the practitioner as to the application of the forceps, it may naturally be expected that I should attempt to remove the difficulty ; I cannot then do better than present the rules which guided us in our practice during the latter portion of Dr. Shekleton's mastership of the Dublin Lying-in Hospital. These were pretty nearly as follows :—

Time was never taken so much into consideration as the existing state of the woman's constitution, and that of the child's circulation. No system of axioms was laid down defining precisely time and circumstances. The *peculiar nature of each case* alone formed the basis or ground of action. But if we *had any axiom upon which we acted, it was this, and this only—"in doubtful cases, and where the application was not difficult, the error was on the right side to deliver with the forceps."* We never waited for bad symptoms to set in ; experience had taught us to discriminate the cases in which unpleasant symptoms *might be apprehended;* and if we did apprehend them, the labour was cut short without hesitation. If we found the fœtal heart about to fail after ergot, or otherwise, we used the forceps at once. We never perforated without first trying to extract the fœtus by the forceps ; and we have successfully used it in slight narrowing of the pelvis. We never required to feel the child's ear, deeming it of no utility whatever ; as I have elsewhere observed, it was not felt in the majority of our cases. The above practice is, to a certain extent, corroborated by Dr. Churchill, who, in the last edition of his 'Practical Midwifery,' remarks—"There are many cases in which, from the character of the labour, it may be certainly foreseen that *these symptoms* (alluding to those which practitioners were formerly in the habit of *waiting* for) will arise, and that the woman will not *previously* deliver herself. In all such cases, I would maintain that the forceps

should be used as soon as we feel justified in coming to that conclusion." I may safely add, that acting thus, should we have been subsequently found to have erred, the mistake shall be on the right side. I emphatically assert, and I have had opportunities of forming an opinion, that no harm at least can result in thus acting; that much life will be saved, and much suffering spared to parturient women.—*Dublin Quarterly Journal, Aug.*, 1861, *p.* 60.

109.—ON THE OBLIQUITY OF THE FŒTAL HEAD IN THE MECHANISM OF PARTURITION.

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The object of this paper is to show that the obliquity, or lateral obliquity, of the fœtal head when passing through the brim of the pelvis, described by Nægele, by some of his predecessors, and by his followers down to the latest authors, does not exist in natural parturition; and that obliquity, or lateral obliquity, of the fœtal head when passing through the outlet of the pelvis, not described by Nægele and his followers, does occur in natural parturition.

This word, obliquity, and the alternative expression, lateral obliquity, are terms requiring, for most readers, some explanation; their meaning certainly not lying on the surface. The words ordinarily used to express the state, considered as a fœtal attitude, seem to me far more appropriate, and scarcely susceptible of being misunderstood —viz., lateral flexion.

I have for several years ceased to teach in my class-room the doctrine of Nægele on this subject, and it has long been my intention to publicly claim attention to my opinions, and thus to secure for them a wider scope, as well as careful criticism. And although it is now three years since Dr. Tyler Smith's 'Manual of Obstetrics' was published, it is the stimulus from a first perusal of that excellent compendium that has led to my taking up the pen. I need scarcely assure my professional brethren that it is with the greatest diffidence that I venture to adduce views opposed to those of Nægele, of Dubois, and of subsequent authors too numerous to mention. It is for the profession to decide whether truth lies with them or with me.

But I am not alone and unsupported in the views I entertain. Many authors, indeed, enter at length into descriptions of, and arguments for, Nægele's opinions; but I know of none who has taken up the opposite side with any fulness. At the same time, I have, since writing this paper, found, on reference to the works of Velpeau, Caseaux, and of Dr. R. U. West, that these gentlemen have reached conclusions similar to my own regarding the direct entrance of the fœtal head into the pelvis.

Nægele's teaching is to be found in his original paper 'über den

Mechanismus der Gebert,' published in 1819, in 'Mekel's Archiv für die Physiologie.' I shall quote from Dr. Rigby's translation of it, published in London ten years afterwards. Before doing so, I must premise that the quotations refer to the first or commonest position of the head in labour, and that in the whole of this paper I shall suppose this position to be understood. The doctrines apply to the other positions as well as to the first, and if proved or disproved for the one, stand or fall for the others.

"At the entrance of the pelvis (says Nægele), the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest, is neither the vertex nor the sagittal suture, but the right parietal bone. The sagittal suture is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards and generally somewhat to the left, across into two very unequal segments." (p. 13.)

"The higher the head is . . . the more oblique is its direction; from which reason the right ear can generally be felt behind the pubis without difficulty, which would not be the case if the head had a perpendicular direction." (p. 16.)

"On account of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never, during its passage, coincide with the diameters of the pelvic entrance." (p. 19.)

In further illustration of this doctrine, I shall quote the account of it in Tyler Smith's 'Manual,' the latest British systematic work on midwifery:—

"The right side of the cranium is considerably lower than the left, so that the most depending part of the cranial surface is the protuberance of the right parietal bone. This lateral depression is called the obliquity of the head." (p. 268.)

"The movement of the fœtal head upon its occipito-frontal axis. . . . This movement causes one side of the fœtal head to become lower than the other during the whole progress of labour, after the head has entered the brim, constituting the obliquity of the head." (p. 274.)

If it be recollected that the axis of the child's body, the axis of the uterus, and the axis of the brim of the pelvis, are represented by the same line in the normal or standard condition, or very nearly so, then it is easy to see that this obliquity implies lateral flexion of the child's head, or the approximation of its left ear to its left shoulder. In respect of this lateral flexion, the child's attitude is thus represented as being changed from that maintained in utero before labour. The position of the child's head, as described in this changed attitude, is oblique; that is, the vertical axis of its head is said not to impinge upon the plane of the brim at right angles to it, but obliquely, forming an acute angle looking forwards.

I do not believe that, in normal or standard conditions, any such lateral flexion or obliquity exists, but that Nægele and his followers are in error in this particular, and that the head enters the brim without any lateral flexion, and directly—that is, with its vertical axis at right angles to the plane of the brim.

I.—The first and chief reason for denying the existence of obliquity of the fœtal head at the brim of the pelvis, is, that it is not observed. I have been in the habit of carefully making out the position of the fœtal head in the brim of the pelvis at the beginning of labour; and, although I have observed that it varies to a slight extent in different cases, I have satisfied myself that it enters the brim, very generally, directly and not obliquely.

The obliquity which is described has never been seen. It is only a supposed result of the observation of many particulars in many cases; and the observations are of a kind demanding great information and carefulness. The liability to be misled by preconceived theories is very great; and I cannot help thinking that the obliquity under discussion is the fruit of a mind powerfully impressed with the important part that obliquity undoubtedly plays in other departments of the mechanism of parturition.

I have not found the sagittal suture nearer the promontory of the sacrum than to the symphisis pubis. I have not found more of the right parietal bone approachable by the examining finger than would be so if the head entered the brim without obliquity. I have not found the parietal protuberance lying near the centre of the brim of the pelvis, nor approaching to it. I have not found the right ear of the child easily reached, while the vertex of the child was at or near the brim of the pelvis.

It is needless to pursue a career of assertions opposed to the statements of the believers in the obliquity. I shall proceed to evidence corroborative of the position I maintain. But before doing so, I shall simply mention that in cases in which, for various reasons, I have introduced my hand, and felt the whole head as it lies in the brim, I have not found the obliquity under discussion. This is valuable evidence, especially in cases where there is no deformity of the head or structures in the neighbourhood.

II.—The descriptions of the oblique position of the head given by Nægele and others are more truly applicable to its direct than to its supposed oblique position. This will be best shown by an examination of Nægele's remarks bearing on this matter.

"*The finger* (says he) *which is introduced in the direction of the central or middle line of the pelvic cavity, and brought in contact with the head, will touch the right parietal bone in the vicinity of its tuber.*" It is to be remarked, *firstly*, that the finger is not introduced in the central line of the pelvic cavity, but only in the direction of it; *secondly*, that it is not made evident that the head is to be touched or approached in the direction of the axis of that part of the pelvis

where the head is lying—that is, in the axis of the brim. The finger may approach the head in the direction of the axis of the brim, but it is scarcely long enough to do so in the axis of the brim, or as nearly in it as, in the circumstances, can be guessed. The consideration of these points shows Nægele's statement to be so vague as to be without much value in this discussion. But it will be evident to all that the quotation cited is perfectly true, on the supposition that the head enters the brim directly, not obliquely.

To bear satisfactorily upon this question, Nægele's statement should have been to the effect, that the finger approaching the child's head in the axis of the brim, or where the imaginary axis of the brim passes through the surface of the vertex, touches the tuber of the parietal bone or its vicinity. Now, while I admit that the finger introduced in the direction of the axis of the brim, as may be done in vaginal examinations, touches the cranium near the tuber parietale, I assert that, on the other hand, the finger carefully introduced in the axis of the brim touches the cranium in or near the sagittal suture, and at a point in its length varying according to the degree of flexion of the head.

"*At the entrance of the pelvis* (says Nægele) *the head does not take a perpendicular, but a perfectly oblique direction, so that the part which lies lowest or deepest, is neither the vertex nor the sagittal suture, but the right parietal bone.*" Now, it is evident that, at the entrance of the pelvis, the head does not take a perpendicular, but a perfectly oblique direction. It does so because the whole child is lying obliquely; and to enter the brim of the pelvis directly—that is, in the direction of the axis of the brim—it must take a perfectly oblique direction. If it took a perpendicular direction—that is, a direction at right angles to the horizon—it would necessarily enter the pelvis with obliquity; but it takes an oblique direction in order to enter the opposed oblique brim of the pelvis directly, that is, in the direction of its own axis and of the axis of the brim.

Further, as Nægele says, the part which lies lowest or deepest is neither the vertex nor the sagittal suture, but the right parietal bone. All this is a necessary consequence of the direct entrance of the head. No doubt it may also be a consequence of the oblique descent of the head, but its occurrence is no proof of the obliquity. The direction of the head entering the brim is nearly that of a line striking the horizon at an angle of 30 degrees. This is a very considerable obliquity to the horizon, but is perpendicular to the brim of the pelvis, which is inclined to the horizon at an angle of 60 degrees. To enter the brim of the pelvis obliquely, the child's head must advance horizontally, or in a line of direction striking the horizon at an angle less than 30 degrees. Nægele does not say that the part which lies lowest or deepest in the brim is the right parietal bone. If he intended to say that, I am at issue with him; and appeal to observation in support of my assertion, that both parietal

bones enter and pass the brim simultaneously, both being in the plane of the brim at the same time.

"*The sagittal suture* (says Nægele) *is much nearer to the promontorium of the sacrum than to the os pubis, and divides the os uteri, which projects backwards, and generally somewhat to the left, across into two very unequal segments.*" The position of the sagittal suture in regard to the promontory of the sacrum cannot be discovered by an examining finger, the parts being too distant to be reached in that way. When the hand is introduced into the vagina to feel the whole relations of the parts before the fœtal head has passed the brim; the sagittal suture is not found nearer the promontorium than to the pubic symphisis. I am thus, at this point, quite at variance with Nægele, and I may venture to point out the cause of his error. It is my opinion that it has arisen from not making the observations relied upon, while the fœtal head was at the brim of the pelvis, and then only; for, after the head has passed the brim and entered the pelvic cavity, the sagittal suture is generally found nearer to the sacrum than to the pubis; and this is not very unfrequently observed even before the os uteri is much dilated, or the labour has been long continued. This approximation of the sagittal suture to the sacrum arises from the descent of the head in the axis of the brim, which coincides with the axis of nearly the whole upper half of the bony pelvis. This axis, when prolonged, strikes the sacrum at or near its point. The fœtal head has a tendency to advance in this axis, and does so till it is arrested by the posterior wall of the pelvis. While it is passing between the symphisis pubis and the two upper bones of the sacrum, it has the sagittal suture equidistant between them; but afterwards, and until the head begins to advance more or less forwards, the sagittal suture approaches to the sacrum, as it descends in an axis which leads it in that direction.

The last part of the quotation just given from Nægele has very little value, for two reasons,—viz., because the situation of the os uteri is far from being fixed or invariable, and because no observations have been made even as to what is its most ordinary position with minute exactness. Nægele himself mentions a frequent deviation to the left, which has not received much notice from subsequent authors. While the head is at the brim of the pelvis, it is difficult to examine satisfactorily the relations of the sagittal suture to the moderately dilated os uteri; and the greater facility of reaching and examining the right and anterior than the left and posterior parts of the head naturally leads, and I believe has led, to exaggeration of the extent of the anterior half over the posterior half of the part corresponding to the circle of the os uteri. But it is out of place to pursue this particular point further, because its settlement would prove nothing, and lead only to the further question of the exact position of the os uteri in early labour, for the ascertainment of which we have no satisfactory data.

"*The higher the head is* (says Nægele) *the nearer its long diameter corresponds to the lateral diameter of the pelvis, and the more oblique is its direction; from which reason the right ear can generally be felt behind the pubis without difficulty, which would not be the case if the head had a perpendicular direction.*" In regard to the increase of obliquity according to the height of the head, I have not one word to say, except that it is a mere statement on Nægele's part, unaccompanied by any corroborative evidence. As I do not believe in the obliquity at all, I can find no place for this refinement.

Nægele's assertion regarding the right ear is quite as much in accordance with the theory of the direct entrance, or entrance in a perpendicular to the brim, as with his own theory of obliquity. I must confess myself completely at a loss, however, as to the full bearings of his argument; for I know well that under no circumstances is the right ear felt behind the pubis without difficulty, and that its position when felt, and the forcing of the finger between the pubis and the head in order to reach it, indicate the direct, not the oblique, entrance of the fœtal head into the brim of the pelvis.

III.—The third reason for rejecting the theory of obliquity at the brim of the pelvis is based upon a careful study of the production of the caput succedaneum, and of the relations of this swelling to the presentation,—a subject in regard to which much has been assumed without evidence, or in defiance of it.

"*Under certain circumstances* (says Nægele), *a swelling of the integuments of the head frequently forms soon after the os uteri has begun to delate . . . . This swelling is situated upon the right parietal bone, close to its upper edge, and equally distant from both angles: a small piece sometimes extends over the sagittal suture unto the other parietal bone; its circumference depends upon the degree of dilatation which the os uteri had attained.*" This statement of facts by Nægele may be added to, but cannot, I believe, be controverted. The caput succedaneum of the first stage of labour can be felt and seen to be as described in the passage. The statements in the quotation immediately preceding this last, and in others, is in quite a different position, involving points not of easy observation, and in regard to which there are manifest inducements to error. But, while I agree entirely with Nægele's statements in this last passage, I would make two additions thereto, of which only the second has an important bearing on the subject of this memoir.

1. It is a condition, necessary for the formation of a true caput succedaneum, that the liquor amnii be evacuated, or that it be in such minute quantity as to have no hydrodynamical properties. 2. While the extent of the caput succedaneum of the first stage may be limited to the right parietal bone, it is generally so limited only when it is not well developed. When it is well developed, it is found to extend over the upper part of the left as well as of the right parietal bone; but its greatest thickness is, as a rule, always in the

portion overlying the right parietal bone. Nægele himself mentions the extension of the swelling over the left parietal as an occasional occurrence of which he gives no explanation.

Before further advancing, it is necessary to inquire what evidence is derivable from the caput succedaneum. In answer, it is certain that it only indicates what was the unsupported part of the head,—in the present instance, what part lay over the os uteri. And as the position of the os uteri is uncertain, and denotes nothing exact topographically, so the position of the caput succedaneum will denote nothing exact topographically, or relative to the position of the fœtal head in the brim of the pelvis. For a fuller development of this subject I beg to refer the reader to my paper on this topic in the *Edinburgh Medical Journal* for July, 1860.

But, before leaving this point, I think it advisable to show that, supposing, as Nægele seems to do, that the os uteri occupies exactly the centre of the brim (except deviation to the left), and that the caput succedaneum formed on the part of the head lying over it marks the part lying in the centre or axis of the brim, the indications afforded by this swelling are not truly read off; and that, if truly read off, they indicate direct, not oblique, entrance of the head into the brim.

The caput succedaneum of the first stage of labour is often formed after the head has passed the brim of the pelvis, and is lodged in the upper half of the cavity of the bony pelvis. Were we to be cautious and exact in reasoning, all such swellings should be excluded from the argument, for evident reasons. It is only those formed at the plane of the brim, or very near it, that can, under any circumstances, afford assistance in settling this question. Under the actual deficiency of exact data, we must be content with stating principles. Now, it is evident that the direction of the caput succedaneum of the first stage will be that of least resistance—that is, the direction of the axis of the undilated vagina; in other words, the caput will be thickest where the head is least supported, and may, in other parts within the circle of the os uteri, be so inconsiderable as not to attract notice. Further, and for the same reason, the centre of the caput succedaneum, or the centre of the os uteri, will not correspond with the thickest portion of the swelling, but in this case be behind it, or nearer the left parietal bone. The oblique direction downwards and forwards of the vagina will lead the caput in that direction; and the support given by the posterior wall of the vagina to the posterior half of the space enclosed in the circle of the os uteri will cause thicknes of the swelling over the right, and comparative thinness over the left parietal bone, and displacement of the thickest portion of it forwards in the pelvis, that is, in the direction of the right parietal and away from the left parietal bone.

IV.—A very cogent argument against the existence of this lateral flexion of the child's head, or obliquity to the plane of the brim, at the

commencement of labour, is derived from the impossibility of finding a mechanism to account for it.

If the membranes are still entire, and there is present any considerable quantity of liquor amnii, and if the axis of the child and uterus are parallel to or identical with the axis of the brim, all which conditions are usually found, it is impossible to conceive any cause of the obliquity but a spontaneous lateral flexion of the child's head; and I daresay no obstetrician will support so extraordinary a doctrine as that the child should, without any discoverable cause, and I may add without any desirable object, bend its head towards its left shoulder as it begins to pass the brim.

If the liquor amnii has been evacuated before the fœtal head has entered the brim, or if the liquor amnii be very scanty in amount, then forces produced by the pains, or the lower parts of the womb and adjacent structures, may be imagined to act directly upon the child, and cause the obliquity. But although they may be imagined, they do not, I believe, exist. If the uterus, for example, became, during pains, more nearly horizontal than it ordinarily is, or quite horizontal, then this grave anterior obliquity of the uterus would probably cause the head to present indirectly or obliquely at the brim. But the opposite of this is observed in nature. The uterus, during a pain, becomes, as it were, erect, and to a certain extent corrects any obliquity it may have during relaxation, becoming, when in contraction, perpendicular to the brim of the pelvis,—that is, occupying its axis. It is certain, then, that no anterior uterine obliquity is observed, which might account for the obliquity of the head at the brim. And it is necessary to remark that the obliquity at the brim, if supposed to be produced by anterior obliquity of the uterus, would not be accompanied by lateral flexion of the head as a change in the fœtal attitude. The flexion of the fœtal head which is so often observed in the early part of labour is easily accounted for by the circumstance that the fulcrum of the head—the spinal column—is nearer the occiput than the sinciput; and all forces acting equally on the various portions of the vertex, act with advantage on the sinciput, as it is at the end of a longer lever than the occiput. But in the case of the sides of the head, the right against the left tuber parietale, no such inequality is observed.

In short, no mechanism has been devised to account for the phenomenon, and it is a vain pursuit to seek it, at least on my part, as I deny the existence of what is to be accounted for.

V.—Assuming that the fœtal head enters the pelvic brim obliquely, Nægele claims for this condition a mechanical advantage over the direct entrance. "*On account,*" says he, "*of the oblique position of the head, the greatest width of the cranium (from one tuber parietale to the other), as well as that of its basis, can never during its passage coincide with the diameters of the pelvic entrance.*" (p. 19.)

It is necessary, *in limine*, to state that discussion on this point,

and conclusions in regard to it, can lend no aid to the settlement of the question under consideration in this paper. It is, indeed, quite a work of supererogation to consider at all the advantages presented by an oblique or direct entrance of the head into the pelvic brim, until the previous question be settled, whether the entrance is direct or oblique.

The position apparently offering the greatest mechanical advantages is not always adopted by nature. Mechanical difficulties seem in various points to be sought, instead of mechanical advantages. The whole process of labour is, indeed, beset with difficulties, one of whose objects is, without doubt, to prevent its too easy and rapid accomplishment. One example, germain to the subject of this article, I may adduce from the entrance of the fœtal head into the pelvic brim. It is well known that its entrance considerably flexed has a great mechanical advantage over its entrance slightly flexed or not flexed at all; yet, in spite of this mechanical advantage on the side of the greater flexion of the head, we find that it generally passes the brim slightly flexed or not flexed at all.

Nægele places the mechanical advantages of the supposed obliquity entirely in the dimensions presented to the plane of brim by the transverse diameters of the cranium and of its basis. In including the basis in his statements, he is decidedly wrong. He would, indeed, appear to forget that the fœtus has a neck, the addition of which to the basis, even when the head is laterally flexed, makes the direct entrance of the basis mechanically the most advantageous, and that so evidently that it is really needless to do more than assert it.

There is no doubt that if the fœtal head passes the brim directly, the greatest biparietal diameter (from one tuber parietale to the other) passes it, and that, if the head enters the brim with obliquity, a smaller biparietal diameter supplants the greatest,—that is, in the first position, a diameter measured from below the right tuber to above the left. But Nægele seems, for the moment, to have forgotten that the long diameter of the fœtal head is not in the transverse diameter of the brim, but in an oblique diameter of this part, and that consequently (as well as for other reasons), the diameters of the fœtal head which he wishes to be compared are not the diameters it is necessary to compare, for they do not pass the smallest diameter of the brim. If mechanical advantage in the way of dimensions is to be of service in the mechanism, it must meet the difficulty,—that is, the gain in diminution of dimension must be in the part traversing the conjugate, or small diameter of the brim; and this is not true of the gain spoken of by Nægele. But, in truth, no gain is desiderated in any natural case, and when the comparison of the oblique and direct diameters of the part of the head traversing the conjugate diameter, as suggested by Nægele, is made so as to be true to the mechanical conditions, it is found that no appreciable gain is got from obliquity. This comparison is too difficult to give in words so as to be useful to a reader.

It is necessary to institute it with the fœtal head and callipers in the hands in order to verify it.

There are, however, mechanical conditions of the laterally flexed head, or of the head presenting one parietal bone to the brim, which, if such a position were assumed, would lead to great and perhaps insurmountable difficulties in a labour. For, if the vertex was by any arrangement displaced from its position at the brim, as Nægele describes, it would tend always to be more and more displaced, till an ear, or even a shoulder, descended. And if the canal of the uterus were rigid and contracted enough to resist such unnatural dislocation, the uterine efforts would be directed along the body of the child to its head, at a great disadvantage. Other mechanical evils might be suggested; but it is in vain to raise difficulties, which, if the object of my memoir is gained, are all chimeras.

While I hold it proved that the child's head passes through the pelvic brim directly, I have, before leaving the subject, to point out that, after it has passed through the brim and upper half of the ligamentous pelvis, it does advance obliquely in its subsequent progress,—that is, the head of the child impinges on the planes of the parts of the pelvis through which it is passing, or on planes at right angles to the axis of the lower parts of the pelvis, not directly, but indirectly or obliquely. A mesial part of the fœtus does not first touch these planes, but a lateral point.

In the first half of the head's course through the ligamentous pelvis, a point in or near the sagittal suture is the presenting point. There the caput succedaneum is formed. During this part of its course the head advances in the axis of the brim, which almost exactly corresponds with the axis of the upper half of the ligamentous pelvis. During all this time, if the head is not covered by the cervix uteri, the right parietal bone is the part first and easiest felt; and the further it advances, the more is this the case. As it advances, and passes the first bone of the sacrum, the sagittal suture approaches nearer to the sacrum, or rather to its lower portions, and becomes more distant from the symphisis pubis. When the biparietal diameter of the head has reached the lower boundary of the upper half of the pelvis, it is arrested in its direct progress. The vertex impinges on the posterior wall of the pelvis, and, in its further advance, the head, as a whole, must change the direction of its course. This change of course is too abrupt for the parts of the head to follow it perfectly. Moreover, there is no room in the pelvis for such a degree of lateral flexion as this would imply,—that is, as would be necessary to maintain the head presenting directly to the plane of the pelvis through which it is passing. While advancing at this point of its progress, the presenting part, therefore, is changed. It soon becomes the upper and posterior part of the right parietal bone, instead of, as before, a point in the mesial line of the head. With this point advancing in the axis of the pelvis, it is evident that the sagittal suture or mesial parts are

far removed from it, and consequently that the head is passing through the lower half of the pelvis, the outlet, over the perineum, and through the vulva, more or less obliquely, and not directly. In accordance with this obliquity. the child's head is flexed laterally, or, to be more exact, flexed obliquely,—that is, bent not directly over the right shoulder, but in a direction midway between extension and direct lateral flexion. As it approaches the orifice of the vulva, and rotates so as to bring the occiput nearer to the pubes than it was in the earlier parts of its progress, this flexion gradually approaches nearer to extension; but it does not become direct extension, almost always maintaining an obliquity,—that is, a direction between extension and flexion.

Under two sets of circumstances, not observed in ordinary labours, the presentation of the fœtal head may be direct from the beginning to the end of the process. In the rare cases where the head enters the brim and passes through the whole pelvis with its long axis in the antero-posterior diameter of the passage, the head will offer itself not obliquely, but directly, in its whole course. The presentation, indeed, will only shift backwards upon the child's head as it descends, maintaining always a position in the mesial line. Again, it is possible that the fœtal head may descend directly with its long axis in the transverse diameter of the pelvis, till it makes a complete quarter of a circle rotation, bringing it into a direct antero-posterior position. Such cases are not subjected to the ordinary laws of the mechanism of parturition.

It is to be remarked, then, that in the second half of its progress the head does not present directly, but obliquely, and that it is born with this obliquity. But this last obliquity is unlike the former, in being quite in accordance with Nægele's statement of the phenomena, though he omits to mention this special point, the obliquity which he describes in this portion of labour being the position of the long axis of the head in the right oblique diameter of the pelvis, not the oblique presentation of the head to the plane of the outlet, or other portions of the pelvis through which it passes in the latter parts of its course. Further, not only is there observed this obliquity to the planes of the pelvis, but there is a change in the attitude of the fœtus simultaneously produced. The head is at first, in this second part of its course, laterally flexed to the right with a backward obliquity; and when passing the vulva this is slightly changed, the condition being one of extension, with a lateral obliquity to the right shoulder.

This obliquity of the child's head to the planes of the lower parts of the pelvic passages is not only observed, but is easily explained. In its descent, the head, if of its ordinary size, must follow the direction of the curved axis of the pelvis. It is possible to imagine the presentation continuing direct while the other parts of the mechanism remain unchanged; but there is no room in the pelvis for the great right lateral flexion of the head that would be necessary to maintain

the presentation direct, and the mechanism does not demand it. A certain amount of lateral flexion is made, and this diminishes the obliquity. This moderate lateral flexion is not produced by spontaneous fœtal motion, but by the powers of labour urging the child through a canal which at this part is rigid and contracted enough to force the soft fœtus to adapt itself to its graduated curvature.—*Edin. Medical Journal, Aug. and Sept.*, 1861, *pp.* 127, 245.

110.—AN EFFORT TO SHORTEN THE DURATION AND DIMINISH THE PAIN OF THE FIRST STAGE OF LABOUR.

By Br. B. Fordyce Barker.

Modern obstetricians divide labour into three stages. The first stage is said to commence with the beginning of labour, and to end with the complete dilatation of the cervix uteri; the second stage ends with the expulsion of the child, and the third includes the delivery of the placenta. But there is a precursory period, during which the uterine neck is preparing for the first stage of labour. This period commences in some two weeks, and in others not more than five or six days before labour may be said to actually begin. During this time the uterine tumour becomes sensibly lower, and this change is accompanied with a corresponding change in the symptoms. The respiration becomes free, the action of the diaphragm is less impeded, and the stomach is no longer compressed; while nausea and vomiting, if they have existed in the latter stages of pregnancy, now disappear. At the same time that the thoracic and epigastric symptoms disappear a new order of symptoms, referrible to the pelvic organs, take their place. There is a pressure on the rectum and bladder, producing sometimes frequent desire to evacuate the bowels and bladder, tenesmus and dysury, and increased œdema of the genital parts and lower extremities. Walking often becomes painful and difficult, and sometimes even impossible. During this time, also, the woman frequently suffers from pains, short in duration, and occurring at distant intervals, but gradually increasing in length and frequency. This is very generally the fact with primiparæ. These pains are coincident with the uterine contractions; for if the hand be placed over the uterine globe during the time of the pain, it will be found hardening, and, as the pain disappears, gradually relaxing. In the multiparæ these contractions frequently exist with scarcely any pain. In the primiparæ, it has been remarked, and the experience of most obstetricians will confirm the remark, that when no cause of dystocia exists, the labour is usually much more rapid in those females who have suffered from frequent pains during the last fortnight of their pregnancy.

We are indebted to modern science for the true explanation of these precursory phenomena. It was formerly taught that at the end of the

sixth month of pregnancy the os internum or the upper portion of the cervix uteri began to open, so as to aid in the enlargement of the body of the uterus; and that this spreading at the upper part continued to advance in proportion as the term of gestation approaches, and consequently, that the length of the neck decreases from above downward, so that the neck entirely disappears at the end of pregnancy. But it has now been established beyond cavil that the neck has no participation with the body in forming the uterine walls to inclose the fœtus; but that, on the contrary, the cervix uteri preserves its whole length until the last fortnight of pregnancy. The neck of the uterus undergoes most important changes during the period of gestation, but a discussion of these changes is not pertinent to the subject of this paper. It is sufficient to say here that the neck of the uterus contributes in no degree to the capacity of the body for retaining the fœtus. But during the last fortnight of pregnancy its length diminishes very rapidly, opening from above downward until it is wholly effaced. The precursory phenomena alluded to above are due to these changes. To use the language of Caseaux, "in the last fortnight the internal orifice softens and yields to distension, then expands from above, so that the upper half of the neck gradually becomes confounded with the cavity of the body; the lower part of the ovum will evidently engage in the dilated portion, and soon come in contact with the parts in the neighbourhood of the external orifice. This contact occasions a progressive irritation of the nervous fibres of the lower half of the cervix, which, by reacting upon the body, excites its contractions, until finally the entire neck being effaced, the irritation reaches its maximum, and labour commences."

The phenomena of the first stage of labour pertain, then, to the dilatation of the *external orifice* of the neck of the uterus. Obstetricians are generally agreed in saying that the duration of this stage is two or three times the length of the second stage, where no cause of dystocia exists. The pains of this stage are of a peculiar character. They are usually borne with more impatience, and very much less fortitude, than those of the second stage. They are greatly dreaded by the patient, who has an instinctive feeling that they are doing no good. She gives utterance to sharp expressions of suffering when the glottis is free and open during the act of expiration. The pains of the second stage seem to be suppressed, like a person carrying a heavy burden, and are scarcely heard except during inspiration. The pains of the first stage are avoided as much as possible, while the patient seems frequently to solicit and invite those of the second. The expressions of the first stage have been characterised as those of *suffering*—the expressions of the second as those of *exertion*. In ordinary language, the term pain in labour is used as synonymous with contraction, and we speak of its return, duration, intensity, and weakness, to describe the force and efficiency of the uterine contractions. But this is by no means strictly correct. The muscular contractility of the

uterus is precisely of the same nature as that of the other hollow organs, as the bladder and the rectum, and is never developed except under the influence of some stimulant or irritant. The exercise of this organic contractility is usually accompanied with pain in the human female, but this is by no means the invariable rule. Dilatation of the upper portion of the neck is very frequently accomplished under the influence of contractions which are not perceptible to the patient; and in some patients the same remark is true in regard to the dilatation of the os externum. The intensity of this organic contractility varies extremely in different females, being very strong in some and scarcely perceptible in others. Its energy bears no relation to the external muscular system; for some strong muscular women have very weak contractions during labour, and some very weak, feeble women have very strong contractions. This organic contractility is, like all muscular power, exhausted by prolonged exercise, as we often see the labour commence with strong, vigorous, and frequently recurring pains, which, after some hours, become slow and feeble, and sometimes cease altogether for a while. For this reason the duration of the first stage of labour may greatly influence the favourable progress and length of the second stage. It is usually said that the first stage may be prolonged without danger, and that the only inconveniences resulting are, the great fatigue caused particularly by the loss of sleep, and in nervous women considerable irritation, depression of spirits, and alarm; while the second stage cannot be prolonged beyond certain limits without greatly endangering the health of the patient, and oftentimes the life of the child.

The first part of this proposition is true, so far as the immediate results are concerned; but it may be quite the reverse from the exhaustion of the muscular contractility produced; thus inducing a feeble and inefficient action of the organ in the second stage of labour.

It is obvious that if the dilatation of the os externum could be accomplished by comparatively painless contractions, as is ordinarily the dilatation of the upper portion of the cervix uteri, a great point will be gained in mitigating "the pains and pangs of labour;" and where no cause of dystocia exists, there is a strong probability that the duration of the second stage would be decidedly decreased.

In 1854, at the close of one of my lectures, I was informed by one of the students that his father, a physician in Ohio, was in the habit of giving belladonna internally as a preparation for labour. I very much regret that I have lost the name of the gentleman, although I have made repeated efforts to ascertain it. The hint given led to a series of experiments, the results of which will be seen in the table of 147 cases of labour, in which I have been able to test the effects of this plan. I have aimed to commence the treatment about two weeks before the end of gestation; but of course the data for calculation are so uncertain, that the length of time that the medicine was taken varied much from this period. The treatment has been tried with

only a part, I may say a small part, of my obstetric patients, for many reasons. In many cases, I have first seen the patient when labour has begun, without having been previously engaged. In others, labour has come on within two or three days after the treatment has been commenced. These are not included in the table. Others, again, I have attended in former labours, and have not been called until the second stage of labour had commenced. Only those are included in the table where the plan has been to a satisfactory degree tested. I have found a very great difference in patients as to their susceptibility to the influence of the agent, and also a great difference in the purity and strength of the article. One extract would seem to have double the potency of another, without any corresponding difference in the appearance, colour, or odour.

I have aimed to commence always with a minimum dose; but in some cases, owing either to the idiosyncrasy of the patient, or the unusual strength of the article, I have been obliged to diminish the dose; but in most cases it has been gradually doubled, or even tripled. The test has been the constitutional effects of the article exhibited in a slight degree; as dryness of the throat, slight uneasiness or giddiness of the head, or dimness of the vision. I direct that any or all of these symptoms should be watched for, and if they appear, to slightly diminish the dose; that is, take it twice instead of thrice a day. In but one case have I had any really unpleasant effects produced. This patient had been poisoned in her childhood by eating the berries of the belladonna, and after taking the article for three days its constitutional effects were suddenly developed, and excited great alarm on the part of her friends. The symptoms, however, disappeared in the course of twenty-four hours. Labour came on the second day after the disappearance of the symptoms, and was so rapid that I did not see the patient until it was completed. Her first labour (this was the second) was reported to have been thirty-six hours in duration.

In not one of the cases was the child still-born, and in none of them was there post-partum hemorrhage or retention of the placenta. In one the function of lactation was entirely absent; and in two others the mammary secretion did not appear until the fifth day. Where the patient was plethoric, of a full habit, rigid fibre and active circulation, I have combined the antimon. et potass. tart., as in the following formula:—

℞. Belladon. ext.	gr. viij.
Antimon. et potass. tart.	gr. iij.
Syr. aurantii	℥ ij.
Tinc. aurantii	
Aquæ	āā ℥ j.

M. S. A tea-spoonful three times a day.

In all the cases, I have commenced with the extract of the belladonna, in one quarter-grain doses. With some I have made use of the following formula:—

℞. Tinct. cinchon. co. . . . . ℥ iij.
Syr. . . . . ℥ j.
Belladon. ext. . . . . gr. viij. M.

Where special indications have existed, I have combined the belladonna with a great variety of other articles. The test of a larger experience of other careful observers is needed to determine whether this prophylactic plan is a real contribution to the progress of the obstetric art.—*Medical Circular, April* 24, 1861, *p.* 290.

111.—SPONTANEOUS EVOLUTION.

[The following interesting case was communicated to the 'Medical Times and Gazette,' by Mr. Heslop, of Haverfordwest.]

A few days ago I was called, at an early hour in the morning, to a patient residing in the vicinity of Haverfordwest, who had been in labour about twenty-four hours. A midwife was in attendance, and upon my entering the house, during a violent bearing-down pain, the right arm passed into the vagina, and was protruding externally through the vulva.

The uterine contractions were very violent, so much so, that I deemed it inexpedient to adopt the usual procedure of turning, until I had so far overcome the powerful contractions of the uterus, by the inhalation of chloroform, as to enable me without danger to turn the child and bring down the feet. On introducing my hand for this purpose, I found the head and arm closely impacted in the pelvis; and having, with much difficulty, passed my hand beyond the presenting part of the child, I was proceeding in the operation, when a violent contraction of the uterus took place, arresting my hand, and changing in a most sudden and complete manner the position of the child. The head and arm, which before were closely jammed in the pelvis, ascended, the breech and the legs simultaneously entered the pelvis, and by one vigorous contraction of the uterus, the child was immediately expelled, thus rendering further manipulation on my part unnecessary. The child was full grown and alive. The right side of the head, face, and neck, &c., were much swollen, although great pressure on these parts could not have continued long without causing the death of the child and endangering the life of the mother.

This case shows how strong were the efforts of Nature to cast off her mould—and as in a *primâ facie* view of the case, turning would appear inevitable, she, by her own unaided efforts, occasionally gives a wonderful illustration of that peculiar mode of delivery termed "spontaneous evolution."

The introduction of my hand, which was difficult, probably elevated in a slight degree the presenting part of the child,—thus beginning, as it were, what Nature so well completed.

The mother and child are doing well.—*Medical Times and Gazette, June* 8, 1861, *p.* 614.

112.—IMPEDED LABOUR FROM DORSAL DISPLACEMENT OF THE ARM.

By J. Jardine Murray, Esq., Surgeon to the Brighton and Howe Dispensary.

Some importance may be attached to the case which forms the subject of this communication, as the peculiar form of obstruction to parturition which occurred does not appear to have been recorded by any obstetrician, except Professor Simpson of Edinburgh. It consists in one of the arms of the infant being displaced backwards across the neck or occipital region; or rather the forearm is thrown across the back of the head and neck, the arm being thrown upwards in a line with the body in order to admit of the malposition of the forearm. In this abnormal position the displaced elbow and forearm of the child increase greatly the dimensions of the head, and form a projecting obstruction which hitches upon the brim of the pelvis and prevents the descent of the head.

Henrietta H., aged 28, had borne two living children at the full term and had one miscarriage at the fourth month. Her first labour occurred five years previously, when she was delivered of a large male child. After an interval of three years she was again confined in so easy and expeditious a manner that the medical man did not arrive till after the birth of the child, which, in this case also, was a well developed male.

At 12 p.m., on March 24, 1859, when Dr. Eastlake was summoned to attend her, he found that she had already been in labour several hours. Liquor amnii escaped, os uteri well dilated, external parts hot. Notwithstanding strong expulsive pains, the fœtal head made no advance, and as there was great prostration, Dr. Eastlake requested me to see her with him. I first saw the patient at 5 a.m. She was much exhausted and delirious, the skin dry, and the pulse weak and rapid. The maternal passages, though swollen, were perfectly relaxed and open, there were strong uterine contractions, the pelvic brim seemed of ample dimensions, and the fœtal head appeared by no means unusually large. It was a vertex presentation in the first position of Nægele. During each pain the fœtal head was pressed down on the pelvic brim, but again receded on the discontinuance of uterine action. The cord was ascertained to be partially prolapsed and perfectly pulseless.

As there was evidently some unusual cause of obstruction, and as it was necessary for the safety of the mother that delivery should be accomplished without further delay, she was placed under the full influence of chloroform. On now passing the examining fingers upwards, I detected the right elbow lying behind the right ear of the child; and on still further pursuing the examination, found that the right forearm lay behind the occiput. After this diagnosis had been carefully and fully verified by Dr. Eastlake, I delivered by podalic

turning. The child was a normally-developed female of average size. The mother made an excellent recovery.

Here, then, we had a case in which, without proper assistance, rupture of the uterus might have occurred, or in which the woman might have died undelivered. The delivery by turning was, as far as circumstances allowed, perfectly satisfactory. In a similar case, it seems highly improbable that the malposition of the arm could be rectified by the natural expulsive efforts. If, therefore, the cause of obstruction were detected early in labour, the accoucheur might possibly hesitate whether to attempt to improve the position of the arm, or to have recourse at once to delivery by turning. But the malposition of the arm which has been described, is not likely to be discovered till the apparently natural labour has been unexpectedly prolonged, for the diagnosis can hardly be satisfactorily made out till the patient has been subjected to the full influence of anæsthetics.

Dr. Eastlake, who took much interest in this case, and who has lately had opportunities of profiting by the extensive practice of the Dublin Rotunda Hospital, entertains a different view of the treatment to be pursued, as may be seen by reference to his remarks appended to this paper. But I cannot agree with him as to the propriety of attempting to deliver by forceps in this form of obstruction, unless the misplaced arm has previously been properly adjusted, for I should have little hope that extraction of the child could be effected by such means. Nor do I think that craniotomy is the preferable operation when the child is dead ; for in cases like that above related, the head has not entered the pelvic brim, which is however of ample dimensions, and the maternal passages are well relaxed. In such circumstances I have not found that delivery by podalic turning, when performed under the influence of chloroform, is an operation of great difficulty or danger to the mother, even after the liquor amnii has escaped.

In the case recorded by Dr. Simpson, the patient had previously had nine easy and speedy labours. His case was also a vertex presentation, but the face was directed towards the left, and not as in the present instance towards the right sacro-iliac synchondrosis, and there was misplacement of the left and not of the right arm.

In all probability, this peculiar form of obstruction sometimes passes undetected. Drs. Simpson and Cumming allude to a case in which "there was no pelvic or other deformity on the part of the mother, no want of uterine contraction, and no disproportionate size of the head of the child, and yet in which the late Dr. Campbell and others entirely failed in extracting the detained infant by the forceps, and were at last obliged to open its head." And it would not be difficult to cite analogous cases from the records of Obstetric Surgery. For instance, the following note appears in the last published record of the practice of the Dublin Lying-in Hospital :—"The first stage occupied about twelve hours. *The head never entered the brim, though the pains were powerful.* Chloroform was administered, and

produced some rest which was much required. The fœtal heart was found to have become extinct soon after the effects of the chloroform were removed, and the head was lessened. The cranium was large and highly ossified, but *no evidence existed of narrowing of the inlet.*"

Remarks on the Foregoing Case. By Dr. HENRY E. EASTLAKE, *late House-Surgeon to the Royal Maternity Hospital of Edinburgh.*

This case cannot but be interesting to all who are engaged in practical obstetrics. I think this posterior displacement of the arm may justly be regarded as a variety of natural labour, in common with hand and head presentation. I quite agree with Mr. Murray in believing that the true nature of this peculiar impediment to the progress of labour may often escape the observations of accoucheurs. The diagnosis of such cases cannot, I think, be ever thoroughly established, unless, as in this case, the whole hand be introduced into the vagina, for it must be at the pelvic brim that the advance of the fœtal head is arrested. The point that becomes so interesting and so important practically is, how such cases should be treated. In the Rotunda Hospital in Dublin the forceps has been successfully applied in two similar instances within the last twelve months; and supposing the fœtal heart to be audible when the true nature of the presentation is ascertained, it seems the most rational practice, although it may perhaps be difficult to explain exactly the mode in which the arm becomes adapted to the maternal passages during the traction of the head. It must, however, be apparent that the life of the fœtus may be placed in great danger, owing to the second stage of labour being unduly prolonged, if we are ignorant of the cause of impediment; and again, the very nature of the displacement may tend to allow of the funis being prolapsed. If, then, from the pulseless state of the cord (should it be prolapsed), or from the cessation of the fœtal heart after its being distinctly audible, or, indeed, from any other source, we can positively diagnose the death of the fœtus, it becomes a matter of importance to determine upon the management of the case under such conditions. The operation of turning cannot, I think, be regarded as the most judicious. First, because, in all probability, the liquor amnii would have long escaped, necessarily involving difficulty, and in some cases impossibility, of version: and secondly, because this operation, performed under such unfavourable circumstances, would, to say the least, be far more hazardous to the mother than either the use of the forceps, or the operation of craniotomy.

The use of the forceps, again, does not seem to be altogether desirable, as the great merit it possesses in giving a chance of life to the infant would be here unavailing. Supposing, therefore, a case in which the infant was dead, I should, in that conviction, be inclined to follow the treatment generally adopted in the Dublin School, when fœtal life is extinct, viz., craniotomy, which is there held to be an

operation *per se*, far less dangerous to the mother than the use of forceps. This is the experience and teaching of Dr. M'Clintock, the present master of the Rotunda Hospital, who has, I think, shown most satisfactorily that the operation is not attended with that amount of danger to the mother generally supposed, and that the great mortality in the statistics of such cases is not due to the operation in itself, but may be fairly attributed to the fact, that craniotomy is often adopted after other modes of treatment, themselves attended by danger, have failed in their object.—*Medical Times and Gazette, June* 15, 1861, *p.* 627.

113.—ON UTERINE HEMORRHAGE.

By Dr. G. Hamilton, Falkirk.

In the 'Edinburgh Monthly Medical Journal' for October 1850, I described a vaginal plug which I had used with success in a case of uterine hemorrhage from cancerous ulceration. Since then I have frequently employed a similar instrument in ante-partum and other cases, and have found it extremely useful. A number of professional brethren, from having read the paper referred to, or from having seen the instrument, have asked me where such a one could be had. I may therefore now mention that I have made some modifications on the original instrument, and that it can be got at Thornton & Co.'s india-rubber warehouse, Princes Street, Edinburgh, as may be seen from an advertisement accompanying the present number of this Journal. This instrument is extremely simple, so that any one can readily get it made or modified so as to suit any particular case. It consists of a brass tube of the length required (say about 3 inches), with two buttons about $1\frac{1}{4}$ inch in diameter, shaped like a watch-glass, the upper of which is fixed to the end of the tube, while the other is movable, and can be pushed upwards by means of a nut and screw working on the tube. At the other end of the tube are slight prominences, to allow of the attachment of a tape or narrow belt. This tube is attached to one of the ordinary vulcanized india-rubber balls, of the size required, by slightly enlarging the hole in it with a pair of scissors, stretching it, and passing the fixed button through the hole, and then forcing the other button up against the ball and fixing it by means of the nut and screw. The tube can be coated with a varnish of gutta-percha dissolved in naphtha, to prevent erosion. The ball used may of course be of any size, but one having a diameter of $2\frac{1}{4}$ inches is most suitable in ordinary cases. Its walls also should not be too thick, as this renders its introduction difficult when the vaginal opening is small. It must, however, be of sufficient strength to re-expand when introduced. When this is attended to, no injection of air or water, which was necessary in the first instruments I used, is now required—the access of air through the tube allowing the ball to expand freely within the vagina. After the ball has been

introduced, it has to be pushed up against the uterus by the stalk, and fixed, with a moderate degree of pressure, by a tape or band attached to it, which has to be fastened before and behind to a belt going round the waist—or what is better, to elastic rings at back and front passed over the belt. In most instances, the mere introduction of the ball, as directed, will be found to restrain the hemorrhage; but in others, where there is inequality of surface from disease, or where the neck of the uterus projects much into the vagina, it is proper, before doing so, to fill up the inequalities with charpie in the former, and in the latter to stuff the upper part of the vagina with lint to the level of the os uteri, and the os uteri itself if necessary. It will be found by the practitioner, in protracted cases of flooding, or where the patient is at a distance, that this little instrument is singularly useful, for the patient herself, or an assistant, can easily be taught to use it. The great advantage of this form of plug seems to me to be, that it allows the pressure to restrain the hemorrhage to be applied *ab externo*. In women who have borne a family, the vaginal relaxation that follows generally renders the introduction of a mere bag useless, from the enlargement of the orifice allowing it to escape, and from the size required causing bearing down efforts to expel it. The stalk in such cases is therefore a necessary part of the instrument. In women, however, who have not had a family, an india-rubber bag, which is not bulky, and which can be inflated after introduction, is sometimes very useful. The little instrument I have described, it will be perceived, also makes a capital vaginal syringe.

In the early stages of labour, with placenta prævia, restraining the hemorrhage is of course a matter of the utmost moment. A case of this kind not having occurred in my practice for many years, I am unable to say whether a plug of the kind described might be used successfully. From the somewhat globular shape mostly assumed by the lower portion of the uterus when labour is commencing, it has struck me that a piece of gutta-percha could easily be moulded into the proper shape, and applied by means of a stalk of the kind described; or that Professor Simpson's india-rubber suction instrument might be adapted to this purpose, applying it to the uterus as he applied it to the head of the child. I have got one of these india-rubber cups fixed on a wooden tube, which I think looks likely to answer the purpose contemplated. When fixed over the os uteri, the external opening of the tube could be closed with a wooden peg, the cup being partially filled, if necessary, with charpie, or with some glutinous substance, such as treacle and dissolved glue. Pressure could be applied either by means of elastic straps, or, if found more effectual, by the hand of an assistant. These latter suggestions are theoretical, and I give them only for what they are worth, as probably each case of this kind will require adaptations suited to its peculiar requirements. —*Edinburgh Medical Journal, Oct.* 1861, *p.* 313.

114.—EXTRA-UTERINE GESTATION.

By Dr. George Cheesman, Surgeon to the Southampton Dispensary.

Though many obstacles, such as are too familiar to medical practitioners in this country, interfered to prevent a sufficiently minute and exact detail of the post-mortem appearances from being obtained, I nevertheless think the case I am about to relate worthy of record. The inspection after death proved unequivocally that the case was one of extra-uterine gestation, of the form termed "tubal;" moreover, it occurred in a married woman not more than forty years of age, who had previously borne four children, and yet up to the time when I was called in, probably eleven months after conception, she had entertained no suspicion that she had become pregnant for the fifth time; lastly, till about six weeks before I saw her she had been, during nearly the three previous months, under the advice of a highly esteemed medical practitioner, who appears to have dealt with the case as one of dropsy of the abdomen.

When I first visited E. S., the subject of this case, she was sitting on the edge of the bed, and presented to me the appearance of a woman who was suppressing the pains of labour. She, however, declared that she was not even pregnant, and had been suffering from the same kind of pain for more than eleven months. Her countenance was anxious, the pulse 96, the tongue clean. I made her stand up, and found the parietes of the abdomen in the erect posture to correspond in every respect to their state in a woman far advanced in pregnancy. In this posture, even firm pressure caused no particular uneasiness. I then asked her to lie down, which she did not accomplish without great pain and difficulty. On the right side she could not rest at all, and the attempt to place herself in the supine position was attended with much pain. It was only on the left side that she could lie, and this had been the case ever since the commencement of her illness. On examination per vaginam, I found the os uteri sufficiently dilated to admit the forefinger of my right hand, while there appeared to be something roundish presenting, which I took to be either a tumour or some part of a child's head. On further inquiry, she informed me that three of her children were still alive, the youngest being eleven years old, and that since the birth of the last-mentioned she had never felt any sign of being pregnant. She said further that since the birth of her youngest child till about Christmas, 1859, she had enjoyed a uniformly good state of health, with no other exception than that there had been a slight irregularity in the monthly periods. She described her illness as having commenced at the time just indicated, with pain in the left side, which gradually increased; that this pain went on augmenting in the same proportion in which her bulk became greater; that the enlargement of her person was very slow at first, but that within the last four months she had

rapidly increased in size. The pain was constant, and extended along the lower part of the abdomen to the opposite iliac region. Since the commencement of her illness menstruation had been scanty, occurring at invervals of five or six weeks, and coming away mostly in clots. She had besides had occasional discharges of an offensive greenish liquid from the uterus. The bowels had been confined throughout. I prescribed an anodyne mixture to be taken at intervals.

Next day I found her lying on the left side with the knees drawn up. There had been some slight abatement of her sufferings, but the pain around the lower part of the abdomen still continued. The bowels had not been relieved; she was annoyed with flatulence, and thought herself troubled with spasms. Another vaginal examination afforded no further information than that of the day before. The naked parietes of the abdomen felt hard on pressure; but at the lower part, below the umbilicus, fluid seemed to be detected by percussion. The umbilicus protruded as in pregnancy; the superficial abdominal veins were much enlarged. A dose of castor oil was ordered, and subsequently to its effect the same mixture was to be continued.

On the third day, Nov. 30th, the bowels had been relieved. The pain was the same as the previous day. She complained of thirst; the pulse was 110, and feeble. The anodyne mixture was continued.

Dec. 2nd. Dr. Welch visited the patient with me. We examined the abdomen with a stethoscope, and we both believed we distinctly heard the sounds of fœtal circulation above the level of the umbilicus, and about two inches and a half to the left. He also made an examination per vaginam, but found the os uteri too little dilated to throw any light on the case. The tongue was now becoming covered with aphthous spots; the pulse was 120, and thready; the countenance anxious. A slight change was made on the anodyne mixture. Beef-tea and wine were allowed *ad libitum*.

3rd. The aphthous spots were enlarged, and extending to the soft palate and to the sides of the cheeks. The patient could retain nothing on her stomach. The pain around the lower part of the abdomen remained much the same. The sounds of the fœtal circulation seemed still audible. An effervescing draught, with excess of ammonia, was added to the anodyne.

4th. There were now great anxiety of countenance and much prostration, without any abatement of the pain. The tongue, cheeks, palate, and fauces were thickly coated with aphthæ; the nausea was undiminished. The sounds referred to the fœtal circulation were no longer audible, while through the parietes of the abdomen the extremities of a fœtus could be felt. The same treatment was continued.

On Dec. 5th there was no improvement, while there was a gradual aggravation of some of the symptoms. The bowels being confined, castor oil was prescribed.

7th. Incessant vomiting of matter, smelling of wine, and having the appearance of feculent substance. It was evident the patient was gradually sinking. The same treatment was continued.

8th. Dr. Palk visited the patient with me. We were both of opinion that she could hardly outlive the day. She died at half-past four a.m. on Dec. 9th.

Autopsy.—On inspecting the body previously to the post-mortem examination, the abdomen was found enormously distended; the parietes were very tense, and percussion indicated a highly tympanitic state of the cavity. The uterine region gave a dull sound on percussion, and there were distinct signs of fluctuation. A section was made in the mesial line, extending from the pubes to the ensiform cartilage. There was much fat, especially over the uterine and sternal regions. When the peritoneum was cut into, a large quantity of most offensive gas escaped, so as to very considerably reduce the size of the abdomen. A distinct movable tumour could now be felt within the abdomen, which might have been taken for the gravid uterus. After a large incision was made, the body of a full-grown fœtus was exposed, lying transversely under the great omentum, with its head towards the right side. By following the funis, the placenta was found, nearly normal in appearance, manifestly involved in a dilatation of the left Fallopian tube. The uterus was found pressed down towards the right side of the pelvis. The uterus was about the size of a child's head, and its walls were thickened to the extent of two inches and a half, its substance exhibiting a fibrous character; the lining membrane presented a congested appearance. Beyond this lining membrane there was nothing in its cavity. The os uteri was undilated. The right Fallopian tube was normal in character. The body of the fœtus was distended with gas, particularly the head, which, from this cause, presented a hydrocephalic appearance. A large quantity—nearly two gallons, of a dark grumous fluid, with flakes of coagulable lymph, was found in the cavity of the abdomen, and dark gangrenous patches observed in several places on the peritoneum.

It must be at once apparent in a case like this, where so many parts are forcibly deranged from their natural positions, how little likely a post-mortem examination, made in haste, is to disclose all the particulars requisite to illustrate its exact nature. I have said that the instance before us belongs to the form of extra-uterine gestation termed "tubal." Some may demur to this decision. But it appears to me that the examination was carried far enough to put this beyond doubt. In the first place, it was not an example of what authors call "ventral" extra-uterine gestation; for, while many deny that any such form of this anomaly can occur, by those who deem it possible it is held to be marked by the absence of any kind of cyst around the ordinary fœtal membranes, and by irregular conditions of the placental development. The dissection, however, in our patient showed unequivocally a firm cyst, which as was remarked at the time, might

have been mistaken for the gravid uterus, while the placenta exhibited nothing abnormal in its character, if its unusual position be excepted. In the next place, that the case in question was not of the form termed "ovarian," or of that named "ovario tubal," seems certain, as no connexion was observed between the ovary of the side concerned and any part of the sac. Lastly, it was not interstitial, for the substance of the uterus was wholly isolated from the sac.

It appears from the statements of authorities on this subjects, that the tubal form of extra-uterine gestation is that which most frequently occurs, and that, as in the example before us, the left Fallopian tube is most frequently the seat of the development of the fœtus. The well-known mechanism of conception, and the experiments made in illustration of that mechanism, render it easy to understand how the arrest of an ovule may occur at any point in the cavity of one of the Fallopian tubes, so as to establish there an extra-uterine development. It would be easy to cite from authorities many well-authenticated examples. The Fallopian tube distended comes to form a thick sac, with walls of a fibrous structure, resulting from hypertrophy of the contracted fibres of the enlarged canal. The natural mobility of the Fallopian tube accounts for the diversities of the position which it occupies when thus loaded with a body, by successive steps more and more developed in volume. Most commonly it has its seat in the hypogastrium, whence it rises to the level of the umbilicus; but at other times, while the bulk is still moderate, the fœtus sinks between the uterus and the rectum, so as to remain immersed in the cavity of the pelvis, beyond the reach of discovery, unless by the fingers being introduced into the vagina or into the rectum.

One of the chief obstacles to the early diagnosis of the real nature of the case before us was the total absence of suspicion of pregnancy, on the part of the patient. This circumstance in respect of a woman who had borne chilldren, and had no reason to conceal the suspicion, if it had arisen, was sufficient to throw a medical practitioner off his guard as to the true diagnosis. It may be presumed, therefore, notwithstanding the evidence of the sensible movements of the fœtus in extra-uterine pregnancies being often both more striking and of earlier occurrence than in natural gestation, that there was here an exception to this rule, such as has been remarked on other occasions of the same character. The apparent continuance of menstruation to which, probably, the patient was altogether unaccustomed in her previous pregnancies, might serve to confirm her in her belief that she was not with child. It is true that authors have laid down the continuance of the menstruation amongst the signs which should lead to a suspicion of extra-uterine gestation; but the statement obtained from the patient in this case, that a discharge took place from the vagina during her illness every five or six weeks, chiefly in clots, is not sufficient evidence that real menstruation was one of its concomitants.

In the early stage of this gestation it seems probable that an exact diagnosis was altogether impossible. In the more advanced stage, however, the stethoscope should have been sufficient to indicate the presence of pregnancy, while the anomalous condition of the os and cervix uteri, and the deficiency of the appropriate weight of that organ as gestation proceeded, should have formed data for the opinion that the development of the fœtus was extra-uterine.

This case not only belongs to a rare kind of anomaly, but as an example of that anomaly is rare in respect of its attendant circumstances. For here the infant obviously lived to the full term of its development, whereas the common rule of such deviations from nature is that the death of the fœtus takes place after the third or fourth month. At this period the sac which supplies the place of the uterus gives way, sometimes with one result, sometimes with another, yet not always with an event fatal to the mother. There were no signs discovered in the post-mortem examination of this woman that rupture of the sac had in any part taken place; nevertheless, the great quantity of altered of blood found in the peritoneal cavity creates a regret that a more minute examination with regard to this point was impracticable.

Th only remedy that could have been available where the place of the fœtus was so distant from the natural passages of parturition was the Cæsarean section, or the similar operation as proposed by Graves, *en deux temps*—that is, the division of tbe skin, the muscles, and the aponeuroses as far as the perineum at one time, and then after eight or ten days, the opening of the cyst and the extraction of the fœtus.

It is true that in extra-uterine gestation it has happened that both mother and child have been saved; but how rarely has this been the issue! How seldom has the operation been performed after the child had reached the full term! And, whatever may have been practicable at an earlier period of this woman's case, I believe no one will venture to say that the Cæsarean section should have been attempted in the state in which she was after the time when I first visited her.—*Lancet, Sept.* 14, 1861, *p.* 250.

115.—IMPROVEMENT IN THE SPECULUM.

Messrs. Weiss have brought out a useful improvement of the vaginal speculum. A serious inconvenience in the use of the bivalve speculum is the liability to pinch up folds of skin or hair between the blades. The practised obstetrist no doubt easily avoids this accident; but it does not fall to every one to use the instrument so frequently as to be able to guard against it. The speculum is shielded by a thin caoutchouc sheath. The speculum itself is materially improved by the action being what is mechanically termed direct, and the mouth of the instrument is bell-shaped. Greater power and perfect inability to strain the instrument are thus gained, as well as a much wider

opening to admit light and favour manipulation. Another improvement consists in the form of the tampon used to facilitate the introduction of this speculum. A caoutchouc bag, attached to a stem, is made to expand so as to overlap the edges of the blades. The inflation is effected by pressing with the thumb upon a communicating bag containing water. The pressure removed, the tampon collapses, and is easily withdrawn when the speculum is *in situ*. We have tried the instrument, and are much pleased with its construction and action.—*Lancet, April* 27, 1861, *p.* 415.

116.—ON VESICO-VAGINAL AND RECTO-VAGINAL FISTULÆ.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[It is interesting to see the progress of surgery simply in the union of injured parts, as well as in more important ways, such as excision of joints, and the radical cure of hernia.]

But of all the triumphs of modern surgery, the cure of vesico-vaginal fistula is perhaps the most brilliant. Ten years ago, most surgeons believed this distressing condition to be incurable. At long intervals one heard of a case successfully treated by caustics or the cautery, or of a spontaneous cure after sloughing and cicatrisation; and occasionally, though very rarely, a cure was obtained by surgical operation. I believe I had been quite as fortunate as other surgeons, having had successful cases both after cautery and the suture between 1845 and 1853. I and my colleague Dr. Savage had each a case in which a small fistula healed after a single silk stitch. I had tried the lead-wire suture, after Dieffenbach; the hare-lip pin suture, after Wutzer; and various forms of silk and metallic sutures, with some encouragement, but I failed after several operations in some cases which I feel confident I could cure now at a single trial. I believe other surgeons were much more discouraged than I was. Many openly expressed their disbelief in the possibility of cure, and attributed the success in cases they could not deny to "luck."

Lecturing thirty years ago ('Lancet,' June 23rd, 1828), Mr. Liston said:—"It was seldom that union took place. All, indeed, might appear to go on well for eight or ten days; but at the expiration of that time the wound probably would be found to have been enlarged by having been interfered with, and would become larger and larger every time the attempt at cure was made. When the communication is to a large extent, but little hope remains to the patient."

So in 1844, Dr. Churchill wrote in his 'Diseases incident to Pregnancy and Childbed:'—"In the majority of cases, I fear we shall find but little benefit. Indeed, vesico-vaginal fistula has long been considered as one of the *opprobria* of surgery, and with some exceptions of late years, the cure has been given up as hopeless." (Churchill, 1844.)

This is but a recurrence to the opinion of Smellie, published now nearly a century ago, who said (Midwifery, vol i, p. 386)—"If the opening is large, close it with a double stitch (the edges of the wound having been refreshed), keeping the flexible catheter in the bladder until it is entirely filled up. I wish this operation may not be found impracticable." (Smellie, 1766.)

Dieffenbach's graphic account of his own disappointments may be taken as representing the feeling of most surgeons ten years ago. In his 'Operative Surgery,' published in 1845, he writes, that all he has said about vesico-vaginal fistula he has learned "bit by bit" from innumerable cases. "I was once delighted to find a fistula extending along the whole roof of the vagina healed in eight days: and I was filled with hope that I was at last able to conquer the enemy at all times; but then I was again defied by a mere needle puncture remaining after an operation otherwise most successful, or a fistula of the size of the head of a probe resisted the most persevering efforts. I saw openings of the size of a small pea, after the use of the suture or the cautery, attain the circumference of a large pea; an opening of the size of a silver penny became as large as a twopenny-piece; one, of a threepenny-piece, as large as a sixpence; and then I stopped. I operated on one woman eighteen times, and did not cure her after all. I collected whole wards full of these unhappy creatures from all parts of the country, and gave up a great deal of time to them, quite liking the trouble, and yet I cured but very few. Two died of cystitis or peritonitis; and one even after suture had led to a complete cure of the fistula."

To show how little faith some surgeons had in any operative treatment of vesico-vaginal fistula, I will just add that Mr. Syme, in the last edition of his 'Principles of Surgery,' published in 1856, does not even allude to the subject. Mr. Fergusson, in his last edition (1857). only gives one line to it; and Mr. Skey in the second edition of his 'Operative Surgery,' published a year later (1858), describes an operation for recto-vaginal and recto-vesical fistula, but does not say one word about vesico-vaginal fistula. I am not saying this to find fault with these teachers. I simply wish to remind you how very recent are the improvements which are now so generally accepted by the profession.

It is from 1852 that what I will call a new era in surgery must date. It is quite true that metallic sutures had been used long before that year. Putting aside all reference to hare-lip pins, and to the entomology pins so largely used in other plastic operations (where the surgeon looked to the support the pins afforded, and the opportunity they give of making lateral pressure upon the opposed surfaces of the wound, rather than to any superiority in metal over hemp or silk), it is well known that Mr. Gossett cured a vesico-vaginal fistula in London as early as 1834 by the use of a suture of gold wire. But his example was not followed. He was even ridiculed for supposing that the

material of his suture could have had any influence on the result. So in 1840, I heard Mr. Morgan, at Guy's Hospital, lecture on the Uses of Platinum Wire as a Suture, and illustrate the non-irritating effects of metal by reference to the manner in which ladies' ears bear ear-rings. But his example was not followed to any extent, either by his colleagues or his pupils. Dieffenbach had been showing how successfully lead-wire replaces silk in many cases; and I applied this wire in cases of vesico-vaginal fistula in 1852 and 1853. Early in 1855, I operated upon a patient of Dr. Simpson's in the Edinburgh Infirmary, and intended to use lead-wire; but an accident put the needle out of order, and I had to trust to silk. Mattauer, of Virginia, also advocated Dieffenbach's suture of twisted lead-wire; but it was left to Sims, of New York, to make known, in 1852, the great use of silver-wire sutures; and to Dr. Simpson, to impress upon the mind of the profession the principles upon which the superiority of metallic over silk and other absorbing materials is established. Next to his discovery of the use of chloroform, I think his exposition of the use of metallic sutures is the best of the many gifts for which medicine and mankind have to thank this great man.

In addition to the introduction of metallic sutures, various modes of fastening and covering them, and of keeping the united surfaces immovable, were also promulgated. I shall allude to some of them presently, although they are now almost disused; and I shall also show you all that is good and useful in various contrivances for exposing the fistula, and paring its edges. But before doing so, I must just allude to one other *principle* upon which success depends. I am not quite sure whether it was Dieffenbach or Jobert who first exposed the error of former operators; but they both insisted upon (what we now wonder could ever have been overlooked) the importance of not cutting away any of the mucous membrane of the bladder in paring the edges of the fistula; and they also showed that if this membrane were perforated by the sutures, the track of each suture would become a new fistula. Hence the obvious rule to carry the sutures *close to*, but not *through*, the vesical mucous membrane; and in paring the edges to spare this membrane. In order to get as broad a surface as possible for adhesion, most recent operators pare the edges in an oblique direction—*bevel* them.

Some operators (Langenbeck some years ago, and more recently Mr. Collis of Dublin) have avoided cutting away anything from the opening, and have obtained their raw surfaces by splitting the border, or separating by horizontal incision the vaginal wall or surface from the vesical wall or surface; in other words, separating the vaginal from the vesical portion of the vesico-vaginal septum. Then, turning the vesical portion of the raw edges towards the bladder, and bringing the vaginal portion downwards, a large raw surface is obtained without cutting away a single shred of tissue. I have adopted this method occasionally; but when the margins of the fistula are hardened, or otherwise much

altered from their natural condition, I think it is better to cut them away. But, whatever plan you adopt, you must bear in mind these two principles:—

1. In paring the edges of a vesico-vaginal or a recto-vaginal fistula, and in passing the sutures afterwards, to spare the mucous membrane of bladder or rectum, and make a broad raw surface;

2. In uniting the raw surfaces, to bring them into close apposition by metallic sutures.—*British Medical Journal, August* 31, 1861, *p.* 223.

117.—ON A NEW AND SUCCESSFUL MODE OF TREATING VESICO-VAGINAL FISTULA.

By Maurice H. Collis, Esq., Surgeon to the Meath Hospital.

[In a former number of the Dublin Quarterly Journal, Mr. Collis drew our attention to some improvements in this operation.]

The operation consists, first, in splitting the margin of the fistula all round, so as to separate the vesico-vaginal septum into two equal portions, one-half consisting of the vaginal mucous membrane and submucous tissue, and the other of vesical mucous membrane and submucous tissue, the muscular portion of the septum being equally divided between the two. The extent of this artificial separation is to be regulated by the extent of the fissure, by the condition of the margins, and to a certain extent by the position of the fissure. Where the fistula is near the vesical end of the urethra, or near the cervix uteri, the dissection need not be carried to any great depth—in the former case from the substantial and highly vascular condition of the parts, which ensures ready union, and in the latter from the toughness of the structure, and also because there would be some risk of opening into the canal of the cervix, an accident which would give rise to a troublesome complication, in the form of a fistula, most difficult to close. On the other hand, when a fistula is situated in the intervening space or floor of the bladder, the dissection requires to be somewhat more extensive, owing to the thinness of the septum, which would otherwise afford too small an extent of raw surface to secure union.

The condition of the margins regulates the amount of splitting in this way—if the margins are unhealthy, or infiltrated, or unnaturally thinned away, the separation of the flaps must be proportionally extensive; if they are healthy, a less amount of dissection suffices. The measurement of the fissure has also a bearing on this point, as stated above; if it be extensive, the flaps must be ample; if small, they need not be large.

The second step of the operation consists in the insertion of the sutures; for my operation either wire or thread will answer; silk is now justly condemned by most surgeons as irritating, and productive of ulceration, as a suture. Good thread has always seemed to me

preferable to iron-wire. I have not an equal experience of silver-wire, and cannot speak to its merits; but I have frequently found iron-wire as irritating in the vagina as silk, although in cases of cleft palate it will remain for weeks unaltered and harmless. I have even gone to the trouble of securing the same kind of wire as Professer Simpson uses, but without having the satisfaction of obtaining the same immunity from sloughing which it seems to possess in his hand. I cannot account for this fact, and only state it that others may not be disappointed. Thread, on the other hand, seems not to produce much local irritation; and I have had thread-sutures lying in the vagina for ten days without producing injurious suppuration. The needles which I use are known as Liston's needles; they are fixed in long handles, with the eye near the point; with these it is easy to pass the thread; and if wire be used, it can be drawn through the flaps by attaching it to the thread. I have my ligatures about a quarter of an inch from each other, and at a considerable distance from the margin of the fistula; this is of great importance. Each suture consists of a doubled thread; and when all are passed through the flaps, a piece of vulcanized India-rubber cord is run through the looped extremities, which are successively pulled tight upon it, and the free ends are then tied over a similar piece of cord at the opposite side of the fissure; the sutures are not tightly drawn. The degree of tightness with which they are to be tied, and the selection of the distance from the margin of the fistula at which to enter them, will test the judgment of the operator, and will most probably insure success or failure as far as he is concerned. If the raw surfaces be tightly drawn together, so that their edges puff up between the India-rubber quills, the flaps will slough. The operator must remember that much swelling will inevitably arise in the course of twenty-four hours, and that the effect of tightly confining the flaps between two bars will be to interfere with their nutrition, and bring about their death. In like manner, if the quills are too close to the margins, their pressure will produce a similar destructive effect. If there be any strain upon the quills, they must produce some slight ulceration where they lie in contact with the vaginal surface; and if the strain be too great, or too close to the margin of the fistula, they will cut through, and cut off the flap. I have seen this happen in other hands, in spite of warning; and it has happened in my own very lately, owing to my using the hemp ligature, which, when wet, contracts most powerfully. I was not aware of this, and was seduced into its use by its beautiful roundness and smoothness, to meet with a result which was by no means agreeable. In my first case I used for quill a piece of soft black bougie, for which, in subsequent instances, at the suggestion of Dr. Thorpe, of Letterkenny, I substituted the India-rubber cord. Its advantages are its elasticity, which enables it to give a little in case of unexpected swelling, and its pliability, which enables us to adapt it to any inequalities of the margins of the fistula. This was strikingly illus-

trated in one case, where the rent was irregularly crucial, and where no unyielding bar would have suited.

Such are the steps of my operation; and I shall now briefly explain in what particulars it seems to be an improvement on the ordinary and older method.

First—It affords an extent of raw surface far and away greater than where the margin is simply pared. It is manifest that any reasonable extent of surface can be obtained in this way; it is only necessary to extend the ring of dissection, to obtain this end; within reasonable limits there is no objection to this procedure. The ridges which might be supposed to result, and which do in fact exist when the cure is complete, will in a few days be smoothed away, and be productive of no permanent inconvenience.

Secondly—This extensive surface is obtained without the loss of a particle of structure, whether in small or large gaps; this is of inestimable advantage. Any one who has removed a ring of mucous membrane from ever so small a fistula, will have observed how large it becomes under the process, and sooner or later will have probable cause to regret the loss of substance thus entailed; whereas, in my operation, if carefully done, no loss of material occurs; and even if it fail, it leaves the patient in no worse condition for subsequent treatment than before. There are, as I shall presently show, cases in which my operation is not expedient, and in these it may be necessary to sacrifice material; but, with these exceptions, I do not think a surgeon is justified in selecting any mode of operating which necessitates the removal of material where there is already a deficiency, and when other methods, which give at least as good results, are offered for his use.

Thirdly—In this operation, we have a double prospect of success. It will be seen, when the raw surfaces are drawn together by quilled suture, that a ridge rises up on the vaginal surface between the quills. A similar and larger ridge is thrown up towards the bladder; this acts as a valve to prevent the water escaping, or even coming in contact with the wound. And even if the parts included between the quills should slough, the flaps which point towards the bladder are exempt from the pressure of the quills and the strain of the sutures, so that they escape from sloughing, and actually unite before the other flaps, and remain united, even if the latter give way.

Fourthly—The operation is simple, and requires no very complex armamentarium, and only that amount of dexterity which should be possessed by every surgeon deserving of the name. I generally prefer the lithotomy posture. It admits of the use of chloroform, and in most instances the fistula can be brought into sufficient command. Sometimes, however, I have found it expedient to adopt another position, viz. placing the patient standing with her back to me, and making her lean across a table, as in the position for examination of the rectum. I use Dr. Sawyer's divaricators, which are of strong brass,

bent at about four inches from the end, at right angles; two of half inch width for the sides, and one of inch width for depressing the rectum. My knives are slightly bent on the flat, an inch from the point, and only differ from those figured by Simpson in being double-edged. One has a lancet point, the other has the angle rounded off; the former is useful in commencing the incisions, the latter in continuing them. However, if such knives are not at hand, any pocket-case will supply the want; for it is not so much the implement as the hand that guides it that secures success; and all surgeons ought to be able to do their work with as few implements and as simple as possible. A good sharp scalpel will do the work as well, if no more convenient knife should be at hand; and a few common iron spoons, properly bent, will make divaricators at a pinch. I have even known an operator pass sutures with common curved needles held in a dressing forceps, when those with which he had supplied himself became bent or broken in the progress of the operation.

Lastly—My operation is suitable to almost every case, and to many cases which could not be subjected to the older methods with the smallest chance of success.

In a large gap, where the loss of substance is to be measured by square inches, no person could expect union by simply paring the edges and drawing them together by the interrupted suture. The strain on the threads would be too great, and they would inevitably cut out. Nor is the success of autoplastic operations, by which flaps are transplanted from neighbouring parts, such as to lead us to expect much from them. In these cases it is of great importance to have a mode of operating which can be frequently repeated without repeated diminution of the already scanty material. In more than one instance I have succeeded in gradually reducing the aperture to moderate dimensions by a succession of operations, and finally have closed it completely. I have invariably included the entire margin in the first operation, as it is impossible to say what part or how much may unite at once. The advantage of this appeared in one case, by the almost complete closure, at one operation, of a fissure which measured three inches in length; a single point only remained open, and this subsequently contracted so as to cause no further inconvenience. In another case, one-third of a large gap closed at the angles; and a subsequent case of almost equal dimensions was rendered manageable by the centre portion uniting firmly, and so subdividing the fissure into two portions, at which I was enabled to work alternately without loss of time as well as without loss of substance.

In small gaps, on the other hand, it will not redound to the credit of the operator, if the rent is made worse each time that he interferes. Such a misfortune cannot happen by my operation in any case to which it is suited. There is no loss of substance; and the surgeon can begin again *de novo* in a few weeks, with the parts in at least as favourable condition as before.

In comparing it with Bozeman's method of operating, it will be found to possess both advantages and the opposite; but these will be more intelligible when I have explained, as I shall now do, the treatment of a case subsequent to operation. The vagina is washed out with cold water; a catheter is passed for a moment to empty the bladder of any few drops of urine that may have collected, and it is then withdrawn until the patient has been placed in bed. If the fistula inclines more to one side than the other, she is placed on the side that is farthest removed from the fistula, with instructions to the nurse to keep her from turning over on her back, as patients are apt to do while recovering from chloroform. As soon as she is conscious, she is similarly cautioned; and is further advised to lean over as much on her face as she can. In this position she is kept for at least two days. The catheter is again inserted immediately on her being placed in bed, and a small vessel laid beside her to catch the urine as it drops from her. I have used various catheters, and find the ordinary male gum-elastic the best, for more than one reason—it is long, soft, pliable, and you can insure its cleanliness by having a new one for each case. Further, I have found the urethra less impatient of it than of metal instruments. In any case, it should be carefully watched for the first day, lest a clot of blood should get into the eye and check the flow of water. In case the urine is not seen to drop from it, it should be gently drawn back and forward once; if the urine does not then flow, it should be withdrawn entirely, cleared out, and re-introduced. At the end of two full days (forty-eight hours), the patient may be turned carefully on her back, the bed having been wheeled opposite a good light, and with the fingers, or with the gentlest and most cautious use of one or two divaricators, the quills and wound may be brought into view with the smallest amount of disturbance possible. If the wound looks healthy; if the flaps are not blue from strangulation (over-tight sutures); if the quills appear not to have corroded the mucous membrane on which they lie, the fingers and divaricators may be carefully withdrawn, the patient turned on the opposite side, a fresh catheter introduced, and all things left as before.

The next day it will be well to repeat the examination, and, if necessary, to cut the loops of the sutures, or at least of the alternate ones. This is best done by touching them with a sharp knife as they cross the quill, cutting them on the quill in fact, and using no force, no pulling or dragging of the quills, and no too curious inspection to see if union is perfect. On the next day the remaining sutures (if any) are to be divided, and one quill removed if loose; and on the succeeding day the remaining quill and sutures will probably be found loose in the vagina, and may be removed. At each inspection it may be well to syringe out the vagina with tepid or cold water. After the third day the catheter had better be withdrawn, when the urethra will be found patulous. The catheter may be introduced morning and evening, to insure the complete emptying of the bladder; or at any

time, if the urine should be felt or seen to cease from flowing. I have found, as a general rule, that it irritates the urethra so much as to induce suppuration, if the instrument be kept in longer than three days; and the suppuration is apt to extend toward the wound, and may even give rise to subacute cystitis. Hence I recommend and practise its early removal. After the quills have been removed, great care is required for two or three days, lest any accident should rupture the soft union. Vomiting, diarrhœa, incautious movements, and accidental injuries by the catheter or syringe, might do irremediable mischief; but without some such cause, there is no likelihood of the union giving way. At first, a thickish ridge will mark the site of the line of union, and two lines of raw surface will show where the quills had lain; but in a very few days these will have healed up, and the former will have disappeared. And I think that we may fairly conclude that the ridges on the vesical surface follow the same course. For a variable period incontinence of urine will probably exist. Of this there are two causes—the patulous condition of the urethra, and the loss of dilating power in the bladder. Both are remediable by time and patience; a few words of advice to the patient to make water often at first, and by degrees to try and retain a little more in the bladder, will counteract both causes of incontinence. For obvious reasons, the patient should be kept in hospital, or at least should not be allowed to return to her household duties until the cicatrix has acquired complete solidity; or if such arrangements be not practicable, strong cautions should be given to all parties concerned to avoid for a time everything which could injure it. An example of the mischief of premature coitus will be found in one of the following cases, where, from the circumstances of the case, the usual prohibition had not been given.

As stated above, there are points in which my operation and Bozeman's may be advantageously compared with one another. Where a surgeon possesses judgment and nicety of touch, he will find my operation afford him excellent results in most cases. He requires both qualities for deciding on the depth to which the splitting process should be carried, the degree of tightness with which the ligatures should be tied, and the moment when to relax them. Bozeman's operation is more mechanical; and though complicated and troublesome, when once the details are mastered, and the apparatus is bought, it needs dexterity of hand alone to insure reasonable success. For reasons I have given a few pages back, it is as little applicable to large rents as the older method, on which it is an ingenious improvement. Its great advantage—and to my mind a very moderate one—is the comparative safety with which the sutures can be removed. The shield protects the cicatrix from injury at the moment of their division; and as they cause no irritation, they need not be removed for several days, until all is perfectly firm. My operation is done much more quickly, and has been completed in periods varying from

twenty minutes to an hour and a quarter. I have heard of Bozeman's occupying thee hours and a half. There is, however, a form of fistula in which I prefer and always perform the latter—where the edge of the fistula is funnel-shaped and puckered. Such fistulæ are small, and generally high up. The condition of the margin makes it almost impossible to apply my plan of splitting; and I find it more expeditious and safer to pare away the mucous lining of the fistula and its funnel-shaped mouth, and to insert wire sutures, and apply Hardy's modification of Bozeman's button. In this we have two parellel rows of holes close together; and the ends of each wire being passed through two neighbouring holes, are simply twisted together, instead of being clamped with shot. I have even applied this plate to the palate in a case of syphilitic loss of substance, in the hope that it would effect a closure. The case was not favourable for success, and only partial improvement followed, but sufficient to show me that most important assistance could be had from a well-adapted button, in suitable cases of fissured palate.—*Dublin Quarterly Journal, May,* 1861, *p.* 302.

118.—ON SECONDARY ABSCESS WITH DISORGANIZATION OF THE KNEE-JOINT. FOLLOWING SCARLATINA AND PARTURITION.

By W. Coulson, Esq., Surgeon to St. Mary's Hospital, London.
(Read before the Medical Society of London.)

The author observed that the case, though incomplete, was interesting from the circumstances under which it occurred. The question raised was connected with the state of blood-poisoning. How far was the fatal pyæmia connected with the puerperal condition? or was it to be regarded as connected with an intercurrent attack of scarlatina? The question whether secondary abscesses are deposits of pus from the blood, or are formed by pus carried from the original fountain of suppuration, was, he believed, now settled; and it was generally accepted that the secondary and local affections in the various forms of pyæmia might be purulent or non-purulent. Thus, in puerperal pyæmia, one joint might be the seat of a copious deposit of pus, while the joint next to it was the seat of apparently common inflammation; and again, these simple inflammations (or at least apparently simple) were just as dangerous as the most copious deposits of pus, both indeed being local manifestations of one cause, and deriving their gravity from that general condition. Pyæmia was now a recognized disease. Formerly it was ignored under the head of "typhoid symptoms." It was now acknowledged to be by no means unfrequent. This, it was contended, depended on the fact, not that the disease was less frequent formerly, but that it was now better known. Who, for example, ever thought, some years ago, of attributing a fatal termination after lithotomy to pyæmia? yet it was now known that purulent infection

of the blood was no uncommon cause of death, not only after lithotomy, but after lithotrity; and records of practice in the last century proved that pyæmia after lithotomy was just as frequent at that period as at the present day. Another interesting point established by a more careful and recent investigation of this subject was the fact that pyæmia, like other blood-poisonings, had its degrees of intensity, and not unfrequently put on a chronic form.

Mr. Coulson then turned to the details of the case. On the 23rd of October, 1860, a woman (Jane A.) was admitted into St. Mary's Hospital, labouring under an affection of the left knee-joint. She was thirty years of age, healthy, and of sound constitution. She had borne five children; and on the 27th of Sept. she had been confined of her sixth child, which was still-born. In a week she was nearly convalescent. At this time her children were attacked with scarlatina, and she slept with one of them. This occurred on the seventh day after her confinement. On the eleventh day, she dressed and got up; but on returning to bed she was seized with excruciating pain in the left knee. Fomentations were first applied; then a blister; but nothing gave her relief. On the fifteenth day after the symptoms first appeared, she was admitted into St. Mary's Hospital, under the care of his colleague, Dr. Tyler Smith. During the whole of her illness she was watched by himself (Mr. Coulson). It was now found that an abscess had formed over the patella of the left knee. The abscess was opened; and subsequently a number of abscesses formed around the joint, and were also opened in like manner. The discharge from them was never very considerable. The patient stated that she had been very feverish at the commencement of the attack; that her throat had been sore, her tongue very red; and that the cuticle had been peeling off during the last month. A stimulant and tonic treatment was adopted from the first—nourishing food, wine, brandy, bark, and ammonia. It was not attended by any marked improvement, though it seemed to retard the progress of the disease. The left lower extremity, however, became gradually contracted; and on the 21st of January last, had assumed a semi-flexed position. Hardly any movement of the joint could be effected; and from the appearance of the parts it seemed probable that secondary dislocation of the tibia backwards had taken place. From this time until the patient's death, which occurred on the 3rd of March, no change particularly worthy of notice took place.

Unfortunately her friends would not permit a post-mortem examination to be made. The body was greatly emaciated. The left lower extremity was flexed, both at the knee and hip-joints. The limb was rotated inwards, and adducted at the hip. On the inner side of the knee-joint was a large sinus, through which could be felt the inner condyle of the femur, denuded of its periosteum. The sinus, however, seemed rather to pass amongst the muscles than to communicate with the joint.

The question naturally arises—What was the nature of the disease, under the influence of which this rapid destruction took place? It could not be attributed to mere local inflammation. Mr. Coulson inferred that the disease had its origin in blood-poisoning, and next inquired, in what manner was the blood contaminated? There was here a choice between two causes, either of which was sufficient to explain the effect produced. The patient had been recently delivered, and it was well known that secondary abscesses and disorganization of joints occasionally occur after parturition. But the patient also laboured under a disease which is occasionally followed by blood-poisoning with its effects. She had undoubtedly been affected by scarlatina, and some would probably consider this as the source of the secondary affection.

Scarlatina, according to the best authorities was often accompanied by a mild articular affection—by synovial inflammation, which closely resembled rheumatic inflammation of the joint, and soon passed away. This form had been described by Dr. Copland, Dr. Watson, Dr. Williams, and more recently by Dr. Willshire; but a more severe form was from time to time observed. Some five or six cases of the kind were on record. There were two forms of arthritis connected with scarlatina—the simple form, and the secondary suppuration of blood-poisoning. The same kind of secondary suppuration might be observed now and then after small-pox, and still more rarely after measles. But the connection was incidental. Scarlatina and small-pox did not give rise to these severe attacks of arthritis in their character as exanthematous diseases, but because they were occasionally attended by local suppurations. The same effects were observed in certain cases of glanders, of dissecting wounds, &c., where the secondary abscesses and disorganization of joints did not occur, unless they had been preceded by suppurative inflammation of the absorbents, by phlebitis, or by some local deposit of pus. In the case of A., Mr. Coulson was inclined to conclude that, although the secondary affection might have found its origin in the scarlatina, it more probably was connected with the puerperal state. The disease, then, under which the woman died was chronic pyæmia. This was a form which he had frequently observed, and in the investigation of which he was an early labourer. It did not necessarily terminate in death, because its chronic character might depend on the smallness of the quantity of poison taken into the system, or on the great resistance opposed by nature; but the cause, course, and symptoms of the complaint could leave little doubt as to the family to which it belonged; yet puerperal women were liable to these attacks of purulent inflammation of the joints, just in the same way as patients were liable to the same accident after injuries of the head, amputations, lithotomy, or other operations and injuries. For a long time the cause of death was not understood in these cases, and the origin of these purulent deposits quite unaccounted for. Various hypotheses were started; but nothing

based on true scientific observation was established until Mr. Arnott took up the subject, and investigated it in a complete manner. Since then many new observations had been made, and general accordance now prevailed on the three following points ——

1st. The poisonous agent in these cases is pus.

2nd. The general symptoms under which the patient suffers or sinks are produced by the admixture of this pus with the circulating fluid.

3rd. The special lesions or secondary deposits found after death are the effects of the general poisoning of the blood.

The author would, however, go further, and say that any disease (as well as lesion) accompanied by the formation of pus, might give rise to the poisoning, provided there was a way for the poison to enter the circulation. The fact was, that in a majority of cases it was not the pus from inflamed cellular tissue which entered the circulation, but pus secreted by the veins in or near the part primarily affected. The immediate cause, in a word, was phlebitis. The train of events was briefly as follows:—First, secretion of pus—a drop or two would suffice. This pus, or some poisonous portion of it, either passed into the circulation or excited suppurative phlebitis somewhere. In either case there was contamination of the blood, with its dangerous and almost always fatal effects—typhoid fever and severe local disease. But great variations occurred both in the course and intensity of the general symptoms, as well as in the secondary effects. The exciting cause was a morbid poison, and it was known from experience—in syphilis, for example, how much the effects and course of such poisons were liable to be modified, partly by the quantity taken in, partly by the constitution of the patient or the weakness of the organs. Thus the poisoning might assume a chronic form, or it might be made up of a succession of subacute attacks, depending probably on successive introductions of pus. In such cases the general symptoms were irregular and obscure. In some, the first symptom had been a violent attack of one or more joints.

Several years ago the author published some fatal cases of articular disease in puerperal females, and even at that early period expressed an opinion, "That contamination of the blood appeared to him the most probable explanation of the varied phenomena of these affections." That opinion need only be modified by adding the word "pus," to be correct according to present views. These attacks were not common or ordinary arthritis. They were developed under circumstances quite different from those which produced other forms of articular disease. The inflammation was often peri-articular, and the pus confined to the exterior of the joint. In other cases we had copious effusion of pus into the joint without any trace of inflammation whatever. This was not ordinary disease. The formation of pus bore no relation whatever to the intensity or duration of the articular disease, as was the case in other affections of the joints.

Besides this, the mere serous form, taken by itself, was just as dangerous in its results as the purulent form. Lastly, the constitutional symptoms accompanying these attacks bore no relation to, and could not be explained by, the arthritic disease, if regarded as common inflammation. We must look, then, deeper for the cause of these affections of the joints in puerperal women, and consider in each case whether the disease be of an ordinary kind, or one which must be referred to blood-poisoning. Articular disease appeared in puerperal women under three circumstances—viz., during puerperal fever, after convalescence from the fever, or after parturition without its having been preceded by fever or other manifest disease. In all cases the disease was the same, though the symptoms were always more or less marked by those of the puerperal fever when that complaint existed. The secondary joint affections were the same under all these circumstances. They might be purulent, articular or peri-articular, acute or chronic. These differences likewise existed in cases of joint disease arising from blood-poisoning after amputations, &c., and this was a very important point to notice. Finally, then, why did he infer that these articular affections of puerperal women had their origin in purulent infection of the blood? Because their symptoms, course, termination, effects,—in a word, all the laws of their manifestation,—were precisely the same as those which prevailed in other cases where the influence of pus-poisoning was universally admitted. Besides, in a majority of cases, pus had been discovered in the uterine veins or lymphatics, or in some of the tissues about the uterus. In chronic cases this original source of the poisoning might not be discovered; it might have disappeared, but it was surely more in accordance with the spirit of philosophy to refer similar effects to similar causes rather than to seek in unknown influences the explanation of a few exceptional cases where direct agency might have escaped detection.

Mr. Henry Lee considered that of all the blood-poisons, none was so deleterious as decomposed fibrin. It will, he said, produce much more formidable symptoms than the introduction of any amount of healthy pus; and he believed that the secondary abscess and typhoid symptoms in the case related by Mr. Coulson, and which had terminated fatally, were to be referred to this cause. After parturition, the healthy veins were blocked by fibrin; scarlet fever then attacked the patient; the healthy action of the occlusion of the vessels was interfered with; a portion of fibrin became decomposed, and was carried into the circulation. He suggested as a question worthy of consideration, whether other kinds of inflammation besides those of a purulent character might not also be followed by their own peculiar secondary inflammations.

Dr. Richardson thought that in these cases of purulent inflammation it was a question—first, whether there was a preliminary state in which pus was formed in an organ or tissue previous to the blood-poisoning; or, secondly, whether the change took place primarily in

the blood? He did not believe in the absorption of pus, and considered that the change originated from within, and not from without; and that pus formed outside the circulation did not enter into the blood. The mere injection of healthy pus into the blood will not produce blood-poisoning. One drachm of pus when thrown into the circulation will not often affect a dog. But supposing the cause to occur from within, and looking on an increase of fibrin in the blood as productive of the peculiar class of inflammations as described by Mr. Coulson, we had in puerperal fever and scarlet fever increase of this element. In puerperal fever there was as much as 9 parts in 1000; and in scarlet fever, 4 in 1000. Hence in these affections depositions of fibrin were liable to take place. He thought that in all probability pus was formed out of fibrin; and he regarded in these cases the formation of abscess as beneficial.—*Lancet, Oct.* 19, 1861, *p.* 375.

119.—OPERATION FOR AND PERFECT CURE OF PROCIDENTIA UTERI FROM PERINEAL LACERATION OF MANY YEARS' STANDING.

By Dr. D. Lloyd Roberts, on the Medical Staff of St. Mary's Hospital for Diseases of Women and Children, Manchester, &c.

Jane W., aged 49, married at 20; admitted an in-patient of St. Mary's Hospital on November 24, 1860; she has had eight children, all born alive at the full period of utero-gestation. The menstrual crisis commenced at 13, without any derangement of health—was always scanty and light coloured, and accompanied by severe dysmenorrhœa. She has always been subject to a yellow leucorrhœal discharge since her marriage. The perineum was lacerated in her third labour, now many years ago, and she attributes the falling of her womb to this circumstance.

Present Condition.—The uterus is completely procident and hanging between the thighs; the inverted mucous membrane of the vagina, which now covers the womb, has assumed almost entirely the character of integument; there are two patches of deep-seated ulceration on the sides of the anterior surface, which are ragged and unhealthy, and encroach on the lips, which are enormously hypertrophied, very vascular, excoriated, and in a state of intense inflammation. The surface of the os and cervix is cracked in two or three places, and a free discharge of blood oozes from the fissured surfaces. The uterus can be returned, though with difficulty, and any attempt to reduce it causes extreme pain.

In consequence of the weight of the uterus, the bladder is pulled down in front of it, and the meatus directed upwards. There is great irritation of the bladder, evinced by frequent desire to void the urine, which is high-coloured and ammoniacal; the perineum is lacerated, the greater portion of the sphincter ani being included in the rent;

and when the bowels are acted upon unduly, as when under the influence of medicine, she loses all control over the passage of the fæces; the orifice of the anus is much dilated and relaxed, and its mucous membrane everted; upon the latter are three small piles. She also complains of dragging pain over the lumbar vertebræ, which is increased on pressure, and extends also down the front of the thighs. There is symptomatic fever, furred tongue, quick pulse, &c.; she is unable either to walk, sit, or stand, and frequently complains that "life is a burden to her." These distressing circumstances, in addition to the miseries arising from poverty, induced me to recommend her to become an in-patient of St. Mary's Hospital.

The treatment, after admission, consisted in the application of soothing lotions to the uterus, so long as inflammatory action remained; grey oxide of mercury, suspended in glycerine, to the ulcers; tonics, as quinia and iron, hydrargyri bichloridum cum cinchona, to reduce the hypertrophied uterus. This treatment was persevered in for seven weeks, at the end of which time the ulcers had healed, and all inflammatory action had ceased. I now turned my attention to the replacing and retaining the uterus in its natural position. I proposed to effect this by the restoration of the perineum, and contracting the calibre of the vagina, by removing slips of mucous membrane from the latter.

Accordingly, on January 5th, 1861, I proceeded to perform the operation, chloroform being administered by my friend, Mr. Runcarn. The steps of the operation were as follows:—The patient having been placed in the position for lithotomy, I passed a needle, armed with a double ligature, through a portion of the mucous membrane on the lower part of the uterus, for the purpose of making traction upon it to facilitate manipulation; I dissected off a triangular slip of the mucous membrane covering the posterior surface of the uterus; brought the edges of the wound together by silver wire; removed a similar piece from the anterior surface, and drew the edges together in the same manner; I now pared the margin of the perineum for half an inch in thickness and two and a half inches in length, brought the edges together by passing two silver hare-lip pins through the whole substance of the perineum, from without to within, and from within to without, after the manner recommended by Mr. Spencer Wells for bringing together the abdominal parietes, after ovariotomy. The operation occupied one hour and a half. The wound was dressed and secured by a perineal bandage, and the patient put to bed. She had a good sleep, and awoke refreshed at four o'clock p.m.

The progress of the case was very favourable, and quite free from bad symptoms. On the eighth day the hare-lip pins were removed, and I was gratified to find that perfect union had taken place along the whole of the united edges, with the exception only of a very inconsiderable portion. This afterwards left the appearance of a small

aperture in the perineum. The ligatures of silver wire on the anterior and posterior surface of the uterus came away at the end of the third week.

The patient was kept for some time rigidly in the recumbent position, to secure strength and permanence of the union of the parts; and was finally discharged on April 20, 1861.

Remarks.—I attribute the successful termination of this case to the following circumstances:

1st. To the strict maintenance of the recumbent posture, both before and after the operation, and also to the therapeutic influence of the hydrargyri bichloridum in reducing the hypertrophied condition of the uterus. I learned the use of this remedy from an excellent paper by Dr. Oldham, in the sixth volume of Guy's Hospital Reports, and have verified its efficacy in a great number of cases which have occurred under my care at St. Mary's Hospital.

2ndly. To the removal of a *sufficient quantity* of the mucous membrane from the vagina to secure its more perfect contraction. Some previous operations of mine have had only partial success, from want of this precaution.

3rdly. To the use of silver hare-lip pins, instead of the ordinary quilled suture: the advantages are, greater facility in use, and a firmer and more secure apposition of the parts.—*Dublin Quarterly Journal, August*, 1861, *p.* 19.

120.—ON ABDOMINAL COLLAPSE.

By Dr. Robert Barnes, Assistant Obstetric Physician to the London Hospital; Physician to the Royal Maternity Charity.

[Shock from severe injury is a common case observed in surgery. It may be noticed also in other instances besides those of external injury. It may occur, for example, from the injury done to the peritoneum and abdominal viscera, when an ovarian cyst is extensively adherent to the neighbouring parts. Dr. Barnes says:]

We see severe injuries inflicted upon a moderate extent of peritoneum, such as surgical incisions for the relief of herniæ, or in the Cæsarean operation, or for the excision of ovarian tumours, make a surprisingly feeble impression upon the nervous centres. But the case is very different when some untoward complication arises that exposes a large superficies of peritoneum to irritation. If in attempting to remove a diseased ovary, the tumour prove to be extensively adherent, then must an enormous surface be subjected to bruising and dilaceration by the hand of the operator; or if, by maladroitness or accident, even a small quantity of the contents of the cyst escape into the peritoneal cavity, the irritation may be fatal. I believe that this is the explanation of a considerable proportion of the deaths from ovariotomy. The fluid of ovarian cysts seems in some cases to

possess a peculiarly irritating quality; there is even reason to believe that it may have a malignant property. The sudden effusion of such a fluid over the entire surface of the peritoneum, whether attended by an incision through the abdominal walls or not, is pretty sure to be resented by the system. This illustration will introduce us to the subject proposed for clinical inquiry.

In January last, I met Mr. Nicholson, of Stratford, in consultation upon the following case:—A lady, aged forty-two, had borne six children, and was at the date of our meeting in the fifth month of pregnancy. She had gone on well until three weeks ago, when she received a kick on the abdomen from a child in bed; she could not fix the spot where the blow fell; she had a little pain afterwards. On the 17th of January, being about eighteen days after the accident, she was seized rather suddenly with great pains in the hypogastric and lumbo sacral regions; shivering, prostration, and fever followed successively. Twelve leeches were applied on two occasions to the hypogastrium with relief, and calomel-and-opium was administered. When I saw her on the 20th, the pulse was 120 and weak; she was lying on her back, the knees drawn up; sighing, or any deep inspiration, increases the pain, which "shoots" from the lumbo-sacral region forward to the groins and pubes; there was excessive tenderness on touch, increasing on deep pressure, especially in the right iliac region; her voice was good, and the countenance not much depressed. The bowels had been relieved two days before, and had been kept quiet since.

Now here we had two leading conditions before us—first, shock and prostration; secondly, some degree of reaction, attended with the ordinary symptoms of peritonitis. What was the cause of these conditions? We shall see by-and-by that there is a whole catalogue of accidents that might produce them. Some of these accidents are, indeed, of rare occurrence; but all the possible events must be present to the mind of the physician if he seeks to arrive at a correct diagnosis in an emergency of this nature, and would hope to bring relief. In the first place, attention would naturally be directed to the pregnant uterus. Had this been ruptured, as has sometimes happened in the course of gestation? A careful exploration by the vagina, and of the fundus through the abdominal walls, satisfied us that the fœtus was still contained in utero, and that the amniotic sac was uninjured. We excluded the idea of rupture of the uterus. The diagnosis given was, that a sudden effusion of blood had taken place into the peritoneum. The source was doubtful. It might proceed from a rupture of the Fallopian tube, or from the ovary. In any case it appeared to have caused peritonitis. Opium was prescribed, and an ointment, consisting of mercurial ointment and belladonna, was directed to be smeared over the abdomen. The result, which is very instructive, I afterwards learned from Mr. Nicholson, who wrote as follows:—"Mrs. J—— lived about a week longer after your visit; throughout, vomit-

ing was excessive, and the pain only remitted at short intervals. She gradually sank exhausted, unable to retain any kind of food. I made a post-mortem examination. The body had decomposed more rapidly than ever I have seen. It was a most frightful spectacle of corruption. To my surprise there was no effusion of lymph or pus; in fact, the only evidence of inflammation of the peritoneum was an increased redness in some parts, but the shining character of the membrane was not impaired. I found a large cyst extending to the under surface of the liver, and discoloured by contact with bile. It proved to be the right ovary; it had burst into the peritoneal cavity; it had contracted no adhesions, and its surface was smooth and shining. A thick, treacly, sanguineous fluid was still escaping, and I had removed, perhaps, a pint of sero-sanguineous fluid previously. The tumour was multilocular, and contained several smaller cysts within the larger envelope. This was dense and thick near the ovary, but thin and yielding upon the slightest force at the loose extremity. The fimbriated extremity of the Fallopian tube was natural, admitting an ordinary probe for some inches. There was a corpus luteum, with its cavity all but obliterated. The uterus was perfectly healthy, containing a five months' fœtus; the membranes were entire, the placenta not separated, even partially; the os uteri closed by a thick mucus, but readily admitting the finger."

A remarkable feature in this case is the absence of the usual post-mortem evidences of inflammation. The patient had survived the first shock of the rupture of the ovarian cyst for at least ten days, and yet there was no plastic exudation or purulent effusion. The persistent pain and vomiting, nevertheless, indicate a continuous irritation of the peritoneum, which in the end destroyed the patient by what may be called protracted shock. From the extreme proneness of the body to decompose, it may also be conjectured that the blood had become poisoned, either by the absorption of fluid that escaped from the ruptured cyst, or by the disturbance of the blood-forming and blood-purifying functions induced by the shock.

Two questions must delay us for a moment before we pass on to other cases. Could the ovarian cyst complicating gestation have been diagnosed during life? Could anything further in the way of medical or surgical treatment have been adopted? The co-existence of a gravid uterus and an ovarian dropsy is not unfrequent. I have known several women who carried ovarian cysts of considerable size go through the pregnant state uninterruptedly more than once. In some of these cases it was possible to trace a furrow of demarcation in the general abdominal enlargement, suggesting the presence of either twins or of two cysts pressed together. You may, of course, infer that there is a twin-pregnancy if you can clearly trace through the abdominal walls the parts of two children, or hear two fœtal hearts beating; but it is not so safe to eliminate or set aside twin-pregnancy if you can detect only one fœtal heart. The history of the case may give you no assis-

tance. The patient may be unconscious of having had any abnormal tumour in the abdomen prior to pregnancy. There is a peculiar cachectic aspect in patients bearing an ovarian dropsy, which may attract attention, but it is not always easy to distinguish this from the cachexia of pregnancy. If, however, we suppose a diagnosis to be established, shall we be justified in tapping the cyst, or in extirpating it, in order to obviate the risk of its bursting under the influence of the double pressure to which it is subjected from its own growth and from that of the pregnant uterus? The case before us proves that the danger is a serious one. I have, I believe, seen it averted in another case by the spontaneous occurrence of premature labour. I think we may exclude ovariotomy during gestation. Circumstances may justify tapping; but I am of opinion that generally it would be the more proper course to terminate the pregnancy first by inducing labour, and then to deal with the dropsical ovary.

But ovarian cysts may cause death not only by discharging their ordinary contents into the abdomen, but also by hemorrhagic effusion. This point I will illustrate by a case, which is taken from the 'Lancet' of 1848. It is related by Mr. Pollard. A single woman, aged twenty-five, but not living in chastity, was taken on the 5th September with vomiting, and pain in the right side. When seen, her face was anxious; anæmic; pulse imperceptible; heart's action very feeble; extremities cold; respiration difficult; collapse complete. She died about twenty hours after the onset of symptoms. The abdominal cavity was found three-fourths filled with blood; there was enough to fill a large chamber-vessel. The uterus was slightly enlarged with a membrana decidua well marked. The uterine appendices and ligaments were much congested; in the right ovary was a distinct corpus luteum. There was a clot in the left tube containing a small cyst, but an ovule could not be made out. Near this clot was a small rent in the tube. The left ovary, as large as a small apple, was hollow; behind it was a rent a quarter of an inch long, in the lips of which was coagulated blood. This rupture had been the source of the hemorrhage. The immediate cause of death was shock complicated with anæmia from the sudden hemorrhage. There was in all probability a Fallopian pregnancy, as well as cystic disease of the ovary; and it is not a little singular that both the tube and the ovary should have been found ruptured together.

There is another way in which fatal collapse may ensue from ovarian disease. I quote a case related by Dr. C. Habit. A woman had aborted, when, without intermediate symptoms, nine hours later, acute pains in the abdomen, vomiting, collapse, pallor, and cold sweat appeared. Soon an ovarian tumour, which had been previously detected, was found to be larger, and its walls more tense. Collapse persisted, and thirty-six hours after expulsion of the fœtus the patient died. On dissection, the peritoneum, especially where it invested the cyst, uterus, and appendages, was injected, dulled, and covered with exu-

dations. The cavity of the cyst, which occupied the left ovary, contained about two quarts of a deep-red fluid and little-altered blood.

Here is another instructive example of the danger attending pregnancy complicated with ovarian disease. Under the stimulus of gestation, the spermatic and uterine arteries carry a largely increased supply of blood; there is an active local hyperæmia. Under these circumstances rupture of vessels connected with the diseased ovary readily occurs. In this case the patient died of shock, anæmia from loss of blood into the cyst, and general peritonitis.

Bleeding into the cavity of an ovarian cyst may also take place independently of pregnancy, as the following case, related by Von Patauban, will show:—A pluripara, aged thirty-two, bore a fluctuating tumour in the hypogastric region, of the size of the uterus in the seventh month of pregnancy. When seen, this tumour was very painful on pressure; her extremities were cold; pulse small and intermitting; pallor. Diastolic after-rush indicated internal hemorrhage. The patient died from repeated attacks. On post-mortem examination, a large dark red tumour was seen emerging from the pelvis; it was stalked, and sprang from the left ovary; it was polycystic, and its cavities were filled with dark fluid blood, five pounds in quantity. The bleeding had arisen from apoplexy from vessels running in behind the tumour, the blood having been retarded through a twisting of the tumour on its axis, caused probably by a fall the patient had suffered shortly before the symptoms set in.

There is no mention here of peritonitis; the patient died of the combined effects of shock and anæmia from hemorrhage. The liability of ovarian cysts to become twisted on their axis, entailing rupture or strangulation of the vessels in the pedicle, must be borne in mind. Attention has been recently drawn to this accident by Rokitansky, who has described several cases.

It must not, however, be concluded that rupture of an ovarian cyst without hemorrhage is necessarily fatal. There are numerous cases on record of cysts bursting and discharging their contents into the abdominal cavity, the intestine, the bladder, or the vagina, in which recovery followed. Perhaps the issue in recovery or in death may be sometimes determined by the innocent or malignant character of the fluid poured out from the cyst.

An ovarian cyst may contain pus which, although not a malignant fluid, may be more irritating than the ordinary serous fluid of ovarian dropsy. Of this we have an instance in a case related by my friend Professor Faye, of Christiania." A woman, aged twenty-nine, had been delivered under chloroform by forceps of a living child. During pregnancy (her first) she suffered much from vomiting; and towards the end she had a fixed pain in the right side of the abdomen, and several convulsive fits. On the night after delivery she had severe pains, which were mistaken for after-pains. Next day these pains were more bearing-down; the abdomen was tender and tympanitic.

She died fifty-three hours after delivery. On dissection, the Douglasian sac was found filled with a thin, purulent, sanguineous exudation. An abscess in the right ovary had burst; the remains of the organ had changed into a mere pulpy detritus. The left ovary was hypertrophied and œdematous. There was considerable degeneration of the cortical substance of the kidneys, and there were many extravasations of blood under the serous membrane covering the kidneys, liver, and lung.

In this case inflammation of the ovary, ending in suppuration, had occurred towards the end of pregnancy; the abscess had burst about the time of delivery. Death was the result of the combined operation of shock. peritonitis, and ichorrhæmia.

To show you the influence of pure shock, I will cite a case related by Dr. Lowenhardt, of death after injecting an ovarian cyst with iodine. A solution, consisting of two ounces of tincture of iodine, two ounces of water, and ten grains of iodide of potassium, was thrown into an ovarian cyst. As much was withdrawn as possible, after being allowed to remain four or five minutes. Collapse set in immediately, and the patient died in fourteen hours. The cyst was found collapsed; there was no trace of inflammation in the cyst or peritoneum. In the cyst was a pint of iodine-coloured fluid, There was also a small quantity of weakly-iodized fluid in the peritoneal sac. In this case it seems clear that death was due to shock alone; there was no inflammation. It can hardly be supposed that death was owing to the absorption of iodine. The quantity remaining in the cyst, or which escaped into the peritoneum, was too small. I have injected iodic solutions into ovarian cysts, and have seen iodism so intense produced, that even the saliva, the breath, and the perspiration yielded abundance of iodine, but the patients recovered from the operation.

In all the cases I have cited, there was disease of the ovary; but this organ may be the source of a fatal hemorrhage into the peritoneum without being strictly speaking diseased.

I cannot on this occasion enter at any length upon the subject of what is called retro-uterine hæmatocele. It will be sufficient for our present purpose to call your attention to this affection as a cause of shock and collapse. Under certain conditions, most frequently connected with disorder of the menstrual function, a considerable effusion of blood may take place from the congested Graafian vesicles and the ovarian vessels; this blood falls into the utero-rectal sac of the peritoneum. If it be poured out slowly, it may set up a benign form of inflammation, the effect of which is to surround the effused mass of blood with plastic lymph, thus cutting it off from the general cavity of the peritoneum. This is called the *encysted* variety. The isolation of the morbid process to a limited area is favourable to recovery. In such cases the shock and collapse may be slight and transient. But if the effusion take place rapidly and in considerable quantity, then will the patient suffer from the combined shock of

extensive peritoneal irritation and of hemorrhage, and from this there may be no rally to give the opportunity of a sequestrating peritonitis. This is the *non-encysted* variety, far more dangerous than the first. This is the form of retro-uterine hæmatocle that more especially concerns us now. The access is sudden, perhaps coming on in the midst of good health, and even suggesting the suspicion of poisoning. Intense abdominal pain, like that of peritonitis, occurs, the patient being thrown into the most violent distress and agitation. The symptoms closely resemble those of perforation of the intestine; but, in addition, we have the blanched countenance, the feeble dicrotous pulse of anæmia from hemorrhage. The belly becomes tender and hard, and dull on percussion. Collapse soon appears, and death follows, not uncommonly in twelve hours, or less.

Such a case may be distinguished from those we have previously passed in review by the symptoms of internal hemorrhage being combined with the absence of any antecedent tumours in the abdomen, and with the presence of a tumour formed by the collection of blood behind the uterus, which may be felt in the roof of the vagina on exploration by the finger.

The cases which we have now passed in review are all connected with various conditions of the ovary. We have seen—

1st. That an ordinary ovarian cyst may rupture during pregnancy, and discharge its contents into the abdominal cavity, causing irritation and perhaps inflammation.

2nd. That an ovarian cyst may be the source of a sudden effusion of blood into the peritoneal sac, causing anæmia, irritation, and perhaps inflammation.

3rd. That hemorrhagic effusion may take place into the interior of an ovarian cyst without rupture, causing irritation from sudden tension of the cyst, and anæmia from loss of blood.

4th. That an ovarian cyst containing pus may burst and occasion irritation and inflammation of the peritoneum.

5th. That a sudden impression upon the interior of an ovarian cyst may cause irritation and fatal shock, as by the injection of iodine solutions.

6th. That under a physiological stimulus blood may be poured out either from the surface of a healthy ovary or from a newly-burst Graafian vesicle into the retro-uterine peritoneal sac.

One thing may have struck us in hearing these cases; I mean, the apparently greater risk of rupture of a diseased ovary under the complication of pregnancy. This circumstance may have some weight in determining the question of the expediency of ovariotomy.—*Lancet*, *Aug.* 3, 1861, *p.* 105.

121.—ON DIPHTHERITIC ULCER OF THE UTERUS.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

[The real diphtheritic ulcer of the uterus is a rare and most intractable disease. It is scarcely noticed in works on diseases of women. Dr. Simpson incidentally mentions its existence. Mr. Ellis says:]

In the elaborate 'Clinical Researches on Ulceration of the Cervix Uteri,' by M. Boys de Loury and M. Costilhes, published in the 'Gazette Médicale' for June, 1845, we have the first account of the diphtheritic ulcer in this region:—"We are not aware that this (diphtheritic) form of ulceration of the cervix uteri has ever been described. Its characters are, nevertheless, of sufficient importance to deserve a separate mention, since it may be readily confounded with syphilitic ulceration, to which, at first sight, it has considerable resemblance. As in the case of diphtheritic angina, the complaint begins with redness and pain; but at this period there is no discharge. In a few days, however, the diseased surface puts on its characteristic appearance, which is that of *small, whitish, smooth, and shining patches, of different and irregular forms.* These patches are extremely adherent to the cervix uteri, so much so that the most forcible injections, or even friction with lint, fails to detach them. At the end of a few days, however, they fall off spontaneously, leaving an ulcer of a simple aspect, which cicatrizes with the greatest facility." From the latter part of this statement would I record my own dissent; for I believe no fact better established as a result of experience than that the diphtheritic ulcer is one of the most intractable lesions the obstetric practitioner has to deal with. And I am much disposed to believe either that these physicians have confounded aphthous ulceration of the cervix with diphtheritic, or that—which is more probable—they have lost sight of their patients before they were really cured; for it is a characteristic of this disease apparently to disappear for a time, and then to return again and again with great inveteracy. The diphtheritic ulcer is also casually noticed by Becquerel in his recent work 'On Diseases of the Uterus.'

The existence and characters of the diphtheritic ulcer can only be clearly determined by the use of the speculum. But there is a certain slightly rough and dry sensation communicated to the finger, which, passing over the otherwise polished surface of the os, comes in contact here and there with a diphtheritic patch, which is very peculiar, if not diagnostic. This sensation of the touch is wholly different from that of the ordinary ulcer, the surface of which is lubricated with the viscid secretions of the cervix, giving a feeling of a slippery and granular character. There is some heat of the vagina, and an absence of the thick mucous discharges of commoner ulcerations: but there is a thinner discharge, probably of an acrid character, issuing from the cervix, and moistening the vaginal walls. The os gapes, and its sides

are seen to be deeply ulcerated, the surface of the sore being coated with a whitish membrane. Here and there the membrane has been removed, probably by the point of the speculum, or by a sort of desquamation, and a raw, vascular, bleeding surface is uncovered. One characteristic of the diphtheritic ulcer is that, unlike the ordinary kinds, it exists independently of the ulceration of the os uteri. All the commoner forms of uterine ulcer appear simply as an extension of the ulcerative process, commencing within or at the edge of the os. This, on the contrary, has an insular existence, and patches of white false membrane are visible, scattered here and there over the cervix, of various sizes, from that of a split pea to that of half a hazel-nut. These patches are quite white, have an irregular, sometimes serrated, edge, are not easily rubbed off, but when removed by the forceps, leave an eroded and bleeding sore behind them. If the practitioner think to treat this disease as he would aphtha of the mouth, or diphtheria of the same region, and pencils it over with lunar caustic, he (and his patient) will infallibly repent it; for every touch of the caustic will be the birthplace of a new sore, and lead to a fresh eruption of false membrane. There is no evidence of the ordinary duration of this ulcer, and it might appear possible that it may ultimately terminate in malignant disease.

The constitutional symptoms are not to be distinguished from those of the severer forms of ulceration. There was in one well-marked case, which I will presently narrate, much pain felt night and day in the hypogastric and lumbar regions, and this pain was described as of a peculiar, dragging, tearing character. There is much painful sensibility of the internal parts on examination, and a sense of great weakness after exercise. Menstruation is irregular, the tongue is coated, bowels constipated, and there is a manifest vice in the constitution, which it is difficult to express by mere words. I think it probable that this disease will be most frequently met with at the close of the functional life of the uterus. Itself the evidence of a diminished vitality, it seems unlikely to occur in the youth or middle age of the menstrual function, and still less in the activities of the pregnant state.

The name of diphtheritic ulcer has been retained by me throughout this paper, not necessarily because I consider it analogous to diphtheria of the buccal and pharyngeal mucous membrane, but simply for convenience, and because it with some accuracy defines a disease which, if not specific, is certainly very peculiar, and deserving of closer investigation. I am well aware that diphtheria of the vulva, occurring co-existently with the invasion of other parts by the same disease, has been observed and recorded; but these cases have no relation to this disease. The diphtheritic ulcer is a localized malady, and, so far as I know, has no tendency to diffuse itself beyond the canal of the cervix, the margins and vicinty of the os, and *possibly* the upper portion of the vaginal passage. I can only characterize it as a very intractable sore, not secreting pus, but exuding a plasma, which takes the form

of a false membrane, that after a time desquamates, and is renewed again. The colour of the false membrane is nearly white, and herein it differs from the ordinary colour of fibrinous exudations. This I believe to be due to the exclusion of the oxydizing influence of the air, and also possibly to the effect of the acidity of the secretions of the vagina, which, as Dr. Tyler Smith has well shown, have the property of coagulating albuminoid discharges, and turning them into an opaque white substance. I believe it to be a distinct affection from herpes and aphtha, the only eruptions with which it could be confounded—first, by reason of the peculiar character of the false membrane formed on the surface of the sore; secondly, because of its extreme intractability and rebellion against ordinary treatment; and thirdly, because of the obvious dyscrasis of constitution, of which it is, if not an exponent, at any rate a significant expression.

The treatment of this form of ulceration will probably perplex the practitioner who meets with it for the first time in no ordinary degree. Any application of nitrate of silver in the pencil, or of the ordinary milder escharotics, will by the next examination have produced such an aggravation of the disease, and so great an extension of it, as at once to suggest the doubt as to whether this be really not a malignant instead of a simple and curable disorder. Dr. Tilt, whose casual notice of its existence I can only discover in his Notes on the Treatment of Ulcers in this region (published in the 'Lancet' for Feb. 23rd, 1861, p. 185), says, with much truth—"In diphtheritic inflammation of the neck of the womb and of the vagina, nitrate of silver acts as a poison. In a case now under treatment there is a small patch of false membrane on the posterior lip of the os uteri, and around it are numerous ulcerations. Were I to touch them with nitrate of silver, they would soon be covered with false membranes. Tincture of iodine would not produce this effect, neither would the potassa fusa c. calce; these, therefore, are the best means of curing this most tedious complaint, of which I have seen two instances, and Dr. Bennet three, and he would endorse what I affirm of such cases." This corresponds precisely with my own experience, and I cannot too strongly urge the practitioner to pause before he applies any of the ordinary escharotics of the milder sort to the treatment of this disease.

I believe that an exclusively local treatment is nowhere less successful than in the disease before us. There is a deep-lying fault in the patient's constitution, and the effort must be made to rectify that co-existently with the absolutely indispensable use of local means. Iron, quinine, and chlorate of potash present themselves as the remedies for the restoration of the general health; and an occasional course of cod-liver oil will be useful, if the digestive powers can be got up to the point of its easy assimilation. The local means employed at first must be either very weak or very powerful. Nitrate of silver and the acid nitrate of mercury are positively injurious in the *early* treatment of the disease, but at a later stage they expedite the cure.

Weak injections of alum may do good to a certain extent, and as a preliminary application; but a cure can only be effected by either the potassa fusa or the actual cautery. Several years ago, in the treatment of a case I was wholly baffled, and almost in despair, at witnessing the evil results of ordinary cauterization; and at length, finding the patient was rather the worse than the better for all my diligence, I resolved to apply cauterization by the electric wire. It was at first used over only a small surface, the wire being at white heat from the electric current evolved by a powerful Grove's battery, and lightly drawn over the ulcer. The most gratifying and rapid amendment followed this experiment, and it was consequently repeated once or twice more over all the diseased portions until the false membranes disappeared, and a healthy sore was left, which was then cured by very mild applications of nitrate of silver.

I believe the whole secret of the successful local treatment of this disease lies in this: that the vitality of the diseased surface must be first profoundly modified, as it can only be by either the actual cautery or potassa fusa, and its tendency to throw out this plastic exudation destroyed, and then the case gets rapidly well with very little further assistance. But when the disease has deeply penetrated into the os and canal of the cervix, it will obviously require unusually careful management thoroughly to eradicate it. The determination of this important point of treatment of diphtheritic ulcer, which was published by me several years ago in a little tract on the 'Electric Cautery,' is, I consider, of great practical moment; for it is calculated to save the practitioner a vast amount of useless effort, and the patient, of suffering. As the potassa fusa or potassa fusa cum calce is a far more convenient escharotic than the electric cautery, and is equally efficacious with it, if carefully used, I should recommend its adoption in all similar cases.—*Lancet, July* 6, 1861, *p.* 6.

122.—ON INFLAMED ULCER OF THE UTERUS.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

The indiscriminating association in modern works of the term "inflammation" with "ulceration" of the neck of the womb, has been, in my opinion, a real mischief to uterine pathology. Inflammation may or may not have preceded, or co-exist with, the ulcer; but in many cases presenting themselves to notice all active inflammation is long since spent, and a condition of mere atony and congestion comes to characterize the sore which once took a higher pathological degree. Of this class is the fungous ulcer of my last article. The truly inflamed ulcer is, however, a common disease; and it is, as its name imports, an ulcer itself the seat of inflammation, and seated on an inflamed basis. I am well aware that up to this time a very

decided repugnance has been shown towards the classification of uterine ulcers, founded to a certain extent on the assumed little practical advantage of such distinctions. But it is really of the greatest practical importance to distinguish the diphtheritic or the fungous from the inflamed ulcer, since the whole question of their cure turns upon the right discrimination of the mode of treatment proper for each; and in my judgment the same remark applies to other phases of the ulcerative diseases of the os and cervix.

The inflamed ulcer, in its most characteristic form, is more common in women under than above thirty. Taking that age as the middle period of uterine activities, the inflammatory character of disease in this region begins thence slowly to lose its intensity, and gradually sinks to the vanishing point soon after the cessation of the menstrual function. This is a mere general expression of the fact; there are, of course, many exceptions. By the inflamed ulcer, I mean, an ulcer of the cervix, irritable, red, with small granulations, hot and tender to the touch, resting on a basis which is also red (though not *so* red), swelled, hard, and sensitive. In these marks may be recognised a state of inflammation which has sunk through the mucous and subjacent tissues (supposing the ulcer to have arisen as a result of laceration in pregnancy or from other causes), and pervades the general structure of the uterine neck; or, on the supposition of a centric origin to the ulcer, we are to regard the whole organ (the cervix) as affected with interstitial inflammation and deposit, and the ulcer resting upon it and partaking of the character of its disorders. I believe the inflamed ulcer capable of taking origin out of either of these two conditions. The inflammatory process rises no higher than to the sub-acute, at which point it may remain stationary, with periodical exacerbations; and in process of time it begins to decline, and in the indolent ulcer we have the next stage of the metastasis. Between these phases of ulceration there are of necessity grades of transition, but this is, so far as I have observed, the most ordinary history of the disease. It is so much the less necessary for me to particularise the symptoms of this disorder, as they have been so often and so well given by others, and especially by Dr. Bennet. My purpose is to establish the position of the inflamed ulcer, so that—as is most necessary and important—it may not be confounded with other and later aspects of disease in this locality. I shall therefore merely take down a typical case—such a one as just before writing this article has presented itself to notice, and while its features are still fresh to mind they shall be transferred to this page.

The sufferer is twenty-two; is tall, thin, very pale, and has fair hair and skin; her face is drawn and anxious, and her general appearance is that of one much out of health. Her hands and feet are cold and wet with a constant clammy moisture. She has for several years been under the charge of physicians and surgeons, with only temporary relief. She has taken quantities of quinine, iron, and alum, but

is wearied with perpetually recurring pain and weakness notwithstanding. She has most perseveringly used all kinds of injections, yet her leucorrhœa, her ache in the back, loins, and under the left breast, have never left her—have only sometimes been a little relieved. She was married at seventeen; in a year bore one child; and from that date has never had a day's real health, and has had no other children. She has wearying pains in her limbs; and a perpetual thick, slightly yellow discharge, "very different from the whites." She stands in a peculiar lolling attitude, as if her body had lost all its stiffness, and she leans for support anywhere rather than transmit its weight to the pelvis. She has pain during and after intercourse, and much pain at the monthly period—the discharge, though scanty, being pale, and hanging about for six or seven days. She is weary of her miserable state, and begins to despair of a cure. A local examination was never instituted.

To the finger the vagina is somewhat hot and tender—most hot at its two extremities—most tender at the cervix. The cervix is deeply lobulated; its anterior is separated from the posterior lip by a wide sulcus, as if in the child-birth it had been laterally torn open, and had never subsequently reunited—which is most probably true. There is a rough and granular feeling over a part of the cervix, which is hard, almost stony, to the touch. The os is open enough to admit the point of the finger, and the finger returns from the exploration bathed in thick mucus and stained with blood.

The speculum has disclosed a state of the cervix which is truly pitiable. Of a bright brick-red colour, it projects its tumid and glossy lips into the instrument, and gives exit by the os to a rope of clear, yellowish viscid mucus, in the core of which is a drop of dark blood. An angry-looking ulcerated patch occupies both lips, studded with bright-red points, and terminating by a most distinct livid-red line, about a twelfth of an inch in diameter. Beyond this the cervix is inflamed, tumid, and tender to the touch. So far as the thick mucus permits the canal to be examined, it is seen to be very vascular, covered with small red granulations, which bleed a little when the cotton wool is used. The whole canal of the vagina is tender, and the instrument, though a small one, is borne with some impatience. The application of the caustic gives rise to a deep and overpouring pain, and an irregular, opaque, white mark indicates the extent of the ulceration. The heat, the greater degree of tenderness, the character of the mucous discharge, the colour of the cervix and granulations, and the tense and indurated character of the cervical structure, are the diagnostic marks of the inflamed ulcer, and they contrast strongly with the lineaments of fungous ulceration which were recently drawn in these pages.

The true explanation of this particular case is to be found in its history. The first pregnancy awoke the disease in a person otherwise of but feeble constitution. The ulcer did not originate out of an

antecedent inflammation, but the inflammation is a result of the ulcer. The cervical structures are infiltrated with lymph, and at a later period this becomes organized to such a degree as to give an almost stony hardness to the whole of the cervix. The inflammatory process is most active around the canal and in the immediate vicinity of the ulcer, and shades off as it passes to the periphery of the neck. It seldom extends to the body of the uterus, and though the vagina is somewhat hot and tender it is only affected by sympathy with the state of the cervix, and there is not the least evidence of vaginitis. In the case before us this state of things had probably existed for nearly four years, and I have reason to believe this to be a very moderate period. Permitted to go on, it will, year by year, pull down the vital powers of the sufferer, the acute symptoms gradually declining—if the patient be not previously carried off by phthisis, or some intercurrent malady, for which she has no health-capital provided.

This case—occurring as it does in dispensary practice, and consequently offering great difficulties in the way of due home-management—will probably take four or five months to cure; in private practice it would take at least three. But it is capable of a complete, and—considering its previous duration—it may even be said a rapid cure. As four years have been already vainly spent in the sole use of tonics for general and injections for local treatment, and as the patient's health is obviously giving way, the treatment must be reversed; and energetic local means must be adopted to destroy the concealed disease, and then general treatment will complete the work.

The treatment of inflamed ulcer is simple, but it is important to be distinguished from that of the fungous or the indolent sore, and the value of a sound diagnosis will be felt in carrying out any plan for the removal of any of these diseases. The administration of quinine and iron must be at once suspended, and, as the bowels are generally constipated, a little saline medicine, sufficiently active to drain away fluid by the bowels, and thus relieve the congestion of the uterus and lower bowel, must be administered; and at the same time, small doses (one grain) of blue pill, with hyoscyamus and ipecacuanha, continued every night for a week or ten days, will in some cases do much good. Simultaneously with the use of these means, it will be most important to apply several leeches to the cervix, at intervals of five or six days, for three or four times. It is especially in this form of uterine disease that the application of leeches does good. In several others it is a positive error, yet one by no means unfrequently committed. The patient will keep strictly to her couch for a fortnight, and subsequently must lie down as much as possible. Emollient injections of linseed tea, milk and water, or thin arrowroot, will follow the leeching, and be repeated two or three times a day. Careful attention to diet (avoiding stimulants), and the occasional use of tepid hip-baths, will also prepare the patient for the adoption of more energetic local treatment. In about ten or fourteen days from the

commencement of this treatment cauterization may be practised. The solid nitrate of silver is less useful than the caustic nitrate of mercury, but it is valuable as an adjunct to the latter, and may be repeated at more frequent intervals. It is most certain that still more powerful escharotics, such as the potassa fusa or the actual cautery, exercise a salutary effect even upon the inflamed ulcer in any stage; I believe the universal experience of surgeons who have carefully tried this plan will be found to agree on this point; and in dispensary practice there is no alternative but to adopt it almost from the first. There is more delay and much more pain in the cure, but it unfailingly cures at length. After a short use of the milder escharotics, one or two applications of potassa cum calce are eminently useful, and astringent injections may supersede those of the emollient class; and now, the tongue being clean, the ulceration much less inflamed and more healthy, and the cervix softer, the proper time for quinine, and if necessary for iron, has arrived, and very rapid improvement will ensue. The ultimate change in the patient's condition, from that of premature old age, weariness, and wretchedness, to one of restored happiness, vigour, and youth, constitutes one of the most interesting of the silent triumphs of our art.—*Lancet, Aug.* 21, 1861, *p.* 181.

123.—ON REMOVAL OF THE CERVIX UTERI.

By Dr. Robert Greenhalgh, Physician to the Samaritan Hospital for Women, &c.

The safety and desirability of removal of the neck of the uterus in certain abnormal and diseased conditions, may still be regarded as questions *sub judice*. Notwithstanding the success which has attended this operation in the hands of Lisfranc, Hugier, and others,—still, fatal results have ensued, which have made some practitioners regard this procedure with a reasonable dread. It appears that the two following important questions have yet to be decided:—1st. What are the most suitable cases? and, 2ndly. By what means can the removal be most safely effected? As a small contribution towards the settlement of these points, I beg to submit the following facts to the attention of your readers.

I may mention that a similar case, at which I assisted, with equally satisfactory results, has occurred in the practice of one of my colleagues. Mrs. H. applied at the Samaritan Hospital on March 18th last. She stated that her age was 25, and had been married six years, but had never been pregnant. Her complexion, fair and ruddy, was much pitted by the small-pox. She commenced to menstruate when between fourteen and fifteen years of age, and always suffered more or less pain about the back and loins a day previous to the appearance of the discharge, which was free, not clotted, and lasted from four to five days. She has generally enjoyed excellent health.

On one occasion, however, about two years and a half ago, the catamenia continued about eleven weeks, and was attended with great pain; the discharge was bright red, and she is sure "no lumps came away." Before marriage she had no idea that "anything was wrong," but shortly afterwards a "swelling came down," which so interfered with her duties as a wife, and led to such frequent disagreements with her husband, as to induce her to seek my advice. The swelling, at first small, gradually increased to its present size. For the last two years the catamenia have become very scanty, lasting about five days, and always attended with much pain.

A vaginal examination detected the cervix uteri protruding between the labia to the extent of an inch, considerably enlarged, corrugated, but not unduly sensitive to the touch. On passing the finger to the extent of about two inches over the growth, which was clearly an hypertrophied cervix, a line of demarcation, was distinctly felt, which appeared to indicate the healthy from the diseased portion. The sound passed easily through the os into the body of the uterus to the extent of five inches and a-half–three inches beyond the normal distance.

On May 17, after a consultation with my colleagues, I proceeded, while the patient was under the influence of chloroform and in the presence of Drs. Savage and Routh, and others, to remove about two inches and a-half of the cervix with the écraseur, which took about half-an-hour, during and after which scarcely a drop of blood was lost. She progressed most satisfactorily for ten days, when the catamenia appeared, and for the first time without pain, somewhat more copious, and lasting a week.

On June 1 the wound of the cervix was healthy and the os uteri pervious: tepid water to be injected into the vagina twice daily.

On the 10th and 17th the nitrate of silver was applied.

On the 25th she again began to menstruate without pain and lasting five days.

On July 11 a careful examination detected the cervix of normal size and structure, the wound having quite healed; the os uteri was pervious and easily admitted the sound to the extent of three inches.

Some time was occupied in adjusting the écraseur, which might have been avoided by transfixing the cervix with a vulsellum and drawing it beyond the vulva, but it was deemed far more judicious to effect its removal while the uterus was *in situ*, a principle I would strongly urge upon those who undertake similar operations upon that organ.—*Med. Times and Gazette, Sept.* 7, 1861, *p.* 237.

124.—ON THE EFFECTS OF UNDUE APPLICATION OF CAUSTIC POTASH TO THE UTERUS.

[Dr. WHITEHEAD, of Manchester, in a letter to Dr. Tilt, makes the following observation on the result which sometimes follows the application of the potassa cum calce.]

In a number of cases wherein it was applied both to the surfaces of the indurated labia and around their inner circle, the reduction of bulk after healing was attended by total closure of the orificium uteri: so complete was the occlusion, that I found it necessary to re-open the orifice with a lancet, and use means to keep it open until the healing was complete. I have the record of several such cases, some of which have been since fertile, and their parturitions been unattended with difficulty.—*Lancet, June* 8, 1861, *p*, 557.

125.—ON OVARIOTOMY AND ITS RESULTS.

By Dr. Charles Clay, late Senior Surgeon to and Lecturer on Midwifery at St. Mary's Hospital, Manchester.

[Dr. Clay has had as much experience in this disease, if not more, than most medical men. He says that the apparent prevalence of the affection may be that we know it better and pay more attention to it, but he is of opinion that it is and always was a very wide-spread disease. Since his first operation in 1842, 1600 cases have sought his opinion.]

Plans of treatment, with the view of lessening the bulk of the tumour by absorption or otherwise, as by iodine, mercury, &c., without being attended by the slightest possible advantage, yet so far prostrate and depress the physical powers of the system as entirely to destroy any prospects of success in an operation, should the patient seek it, although a fair average of success might have been anticipated if such pernicious treatment had not been put in force. Pressure and galvanic currents on and through the tumour have each been advocated very strongly by some parties. I have heard of no cases where any advantage has been gained by galvanism, nor do I believe any good result could possibly be gained by such process, while there is a probability of rousing into greater activity a disease almost dormant, or very slow in progress at the time. As to pressure, save and except in such rare cases where it can be certain that a single cyst unaccompanied by solid nucleus exists, in such a case it is just within the bounds of probability that success might attend the process; but in all other cases, that is in nineteen-twentieths of the cases I have seen, pressure would not only be unattended by any advantage, but decidedly the most pernicious and injurious practice that could possibly be put in force. Suppose, for a single moment, a large multilocular tumour of many pounds weight (my own cases average 27lbs., and I have extirpated many heavier, and one of 73lbs.,) how is it at all likely that any pressure could ever effect, by *absorptive process*, a disappearance of such a mass in the short time allotted to the lives of individuals suffering under this disease,—a term of not more than three years on the average? The whole affair is not only a physiological absurdity, but an impossibility.

The above are not the only objections. Violent and long-continued pressure for a time, will be as certainly followed by severe attacks of inflammatory action, resulting in extensive adhesions of the tumour to the abdominal parietes, or both of them to the pelvic and abdominal viscera, forming a matted mass of living structures impossible to unravel (proved in a *post-mortem* examination), and presenting insurmountable obstacles to any future attempts at extirpation, which it would be madness to encounter. I have invariably sent such cases back to the parties adopting such plans, that they might see the terminations of the sufferings they had occasioned. Two deaths have come under my notice where the pressure system had been tried to the fullest extent, and the awful sufferings of the poor creatures during the progress of enlargement, where all the parts concerned were matted together firmly (as was proved by examination after death) cannot be described. All the cases where I have seen pressure tried confirm my opinion that the progress of the disease was greatly accelerated, except, as I have before stated, in single cysts, thin walled, and not accompanied by any solid nucleus, but these cases are extremely rare.

From these observations, I conscientiously believe that neither medical treatment, external nor internal applications, pressure, nor galvanism, are of the slightest benefit; they neither cure nor palliate the disease. All such attempts, then, are fallacious, and only throw obstacles in the way of any benefit that extirpation of the tumour offers, increasing the difficulties of that operation, if not defeating it altogether.

I ought in this place, before I proceed further, to admit one exception to this general condemnation, and that is in the case of one large single cyst with thin walls, which by tapping and injecting the sac afterwards with two or four ounces of the strong Edin. tinct. of iodine, might be productive of cure. I have seen three cases succeed by this plan, and as these operations took place now three years ago, the result has fully justified the means employed. Thus, then, in single cyst cases, I think such means ought first to be tried, even to a second or a third injection, before requiring the patient to submit to the process of extirpation. Still, I must not omit to mention that the chances of success of extirpation are not improved by the previous use of iodine; such cases never recover so well as those where iodine or mercury have not been used.

I have spent much time in endeavouring to decide if ovarian disease be more prevalent in young females or in older persons,—in single, or married females. It is a question of great difficulty in the absence of a larger range of statistical information than I or any other single individual could possibly have at command; still, with the information I have been able to obtain, I believe the preponderance is with the young and unmarried females; and I feel also certain that the tumours in those classes are far worse, as a whole, to cope with,

having generally a larger proportion of solid mass, accompanied by more severe symptoms, more rapid as to growth, and, as a matter of course, lead to an earlier termination.

In elderly females, *unmarried*, the tumours are very frequent, and usually not of a very promising character; whilst, on the contrary, elderly *married* females seldom have the disease until after the child-bearing period; that is, after the cessation of menstruation. In such cases, the tumours are slow of growth, multilocular in character, and usually not much adhered; and, I may add, but seldom accompanied by severe inflammatory attacks. It is this last class of cases that I should prefer, before all others, for extirpation; and if I must have a choice of age, I prefer those between forty and fifty to any other. Of course, there may be good cases at all ages, and many good recoveries have been made at all ages; but my experience points out the above cases as most probable for success.

My attention was, contrary to my expectations, very early directed to one fact, which is, that the right ovary is far more frequently the seat of disease than the left, and I am yet unable to account for it. In my earliest cases I was impressed with the idea that the left would be found more frequently the seat of disease; experience, however, proves the contrary to be the fact; taking the whole number of my cases I may venture to say that at least two-thirds have been of the right ovary. With respect to the connexion of ovarian with uterine diseases, I have found about one-fourth of the entire number of cases to be more or less connected with uterine disease. In my 'Manual of Obstetric Operative Surgery,' published in 1856, I stated about one-third of ovarian cases were connected with uterine disease, but I have since found that in that statement the proportion was too large; the present announcement of one-fourth will be found to be nearer the truth. I need scarcely remark that all cases of ovarian disease, accompanied by uterine disease involving its structure, such as hypertrophy, ulcerations, cancer, &c., &c., place the operation of extirpation entirely out of the question, to attempt which, under such circumstances, would be altogether unjustifiable, as it would certainly only hasten the patient's death. Hence the great necessity of defining, which is often a matter of great difficulty, whether the case be accompanied by uterine disease. The large number of cases presented for diagnosis compared with the small number operated on in my own practice, will at once defend me from the charge of seizing on every opportunity for operating; 104 operations out of 1,600 diagnosed cases is but a very small proportion. I could easily have trebled the number of operations, which would have added to my pecuniary advantages, but have preferred, after giving an opinion, rather to be sought after than to urge the sufferers to undertake the chances of an operation.

It might naturally be inferred, from the aptness of the ovaries to take upon them morbid derangements, that we should find both fre-

quently affected at the same time. This is occasionally the case; but not nearly so often as we should be led to suppose. I consider it to happen not more than once in twenty cases. But when *both* ovaries are affected, it is almost always found that the uterus is also involved. Where both ovaries are affected, the operation for extirpation is scarcely admissible, though the uterus may be free, as it calls for the extirpation of both ovaries at once; or another operation will soon after have to be performed on the second ovary. I have had one such case, where a large ovary of 37lbs. was removed, and at the time it was found, the other ovary was enlarged to the size of a walnut. Both were excised, the case did well, and made a complete recovery, but was followed by some curious physiological phenomena, such as loss of sexual passion, loss of voice, at least it became croaky, and many characteristics of the female vanished.

The characters of the tumours vary very considerably; about one-fourth of the whole are of a solid lobular kind, often considerably adherent, slow of growth unless provoked by accidental injury, or irritated by treatment, as by pressure, galvanism, &c. Such a class of tumours are not very favourable for extirpation. Nearly one-half, or more than a third have well developed and more or less large cysts, with some portion of solid nucleus, pretty free from adhesion, are of more rapid growth, especially if any of the cysts should be emptied accidentally or intentionally; these are a class more favourable to extirpation. A still smaller proportion consist of thin-walled cysts, with scarcely any solid material (in some rare cases, of one large thin-walled cyst, with no traceable solid mass). This class is of rapid growth, seldom adherent, and very favourable to extirpation, and occurs mostly with females of younger ages. In such cases it is only reasonable, before extirpation is proposed, to try the injection of strong tinct. of iodine after tapping, particularly where there is but one large cyst, and I would persevere to the second or third time, before proposing extirpation. I have seen three cases making rapid and permanent recoveries by this mode of proceeding, and I have also seen it fail in two other cases. Still it must be borne in mind, that the cases themselves are of the rarest kind, as I have not met with more then ten in the whole of my practice that I could call *single cyst*, and free from solid mass. It must also be recollected that, wherever iodine or mercury have been used to any extent, the chances of recovery from extirpation is considerably lessened. It is, therefore, desirable to use neither, as no good results from their use, except in such cases as has just been stated, where a reasonable prospect of success may be anticipated; although, if it fails, the subsequent operation is deprived of some of its chances of success. If there are more cysts than one, or if a cyst be accompanied by solid material, it would then be the height of absurdity to inject a cyst after tapping, as it cannot, in the slightest degree, check the formation of fresh cysts from the solid mass, or prevent the other cysts already formed from enlarging;

therefore, as it is incapable of influencing the main disease, and would injure the prospects of success in the extirpation, such modes of treatment should be carefully avoided.

Mr. Safford Lee believes that married females are more liable to ovarian disease than single ones; my own opinion is, that the liability is about equal. As respects age, I believe the commencement and decline of menstruation are the periods most productive of ovarian disease, probably because of the irregularities attendant on the commencement and cessation of menstruation. I have said the duration of the disease, on the average, is seldom more than three years; this applies more strictly to younger persons; for, if we divide the cases into those *under* 30 years of age, and those above 30, we should find that with the latter the disease is often of much longer duration than with the former.

The growth or progress of ovarian disease, when once established, forms a very important point of consideration, and often presents considerable difficulty in forming an opinion as to the future prospects of the case; speaking of these cases as a whole, I consider them on the average very rapid in their development, and taking this view, I should not hesitate in limiting the duration to two and a half, or at most three years. Notwithstanding, there are many cases that suffer under this disease for very many years, attaining an enormous size, and some of these cases appear to suffer but little, except from the inconvenience of bulk, and inability to move about. Then, again, some have borne tapping a great number of times without much suffering, whilst others have immediately sunk after the second or third tapping; the former being the characteristic of the disease in advanced life, and the latter that of earlier or younger lives. On the average, then, these cases seldom exceed three years, and if tapping is resorted to as a simple means of relief, the patients generally sink after the second or third operation.

The growth of ovarian tumours appears to be considerably influenced by age, that is, if we divide cases into those above *thirty*, and those under *thirty* years of age, the former are of much slower growth than the latter, and accompanied by much less constitutional disturbance. I have generally found the most unfavourable cases and the most rapid growths during the earlier ages, and arising *rather* from obstructed uterine and ovarian functions during the child-bearing period, than at later periods of life, and more immediately connected with the cessation of the menstrual function. I conclude, then, from these remarks, that there are but few cases which do not complete their termination in some way within three years from their commencement.

I do not intend to occupy the reader's time by speculating on the pathology of ovarian disease; the seat of this disease is still disputed, and, if settled, can in no way alter the means proposed for its treatment, whether that be for the simple alleviation of suffering, or its

radical removal. I would, however, say a few words on the varieties of tumour which present themselves to the practitioner.

And first, the *simple cyst* attached to the ovary, or broad ligament, either by a distinct pedicle of its own, or arising from a broader base. This form of the disease is very rare, and, as I have previously stated, I have seen but very few such in my practice; they are the only cases that are likely to yield to iodine injections after tapping; and in half of such, at least, tapping with injection will not succeed; still, if half succeed it would not be justifiable to neglect a trial. Spontaneous bursting and tapping have cured this form. *Cysts from breaking up of the Graafian vesicles* is a much more frequent form of ovarian disease, these cysts breaking up into each other until no remains of the original ovary are to be found; this class, as well as the simple cysts, are of rapid growth, and often attain a very large size. The fluid contents are generally coffee-coloured, but present considerable variety in appearance. From twenty to thirty pounds of fluid are often taken from cysts of this character, and I have in one case taken seventy-three pounds. In these cases the form of the tumour is more unequal or lobular than in the single cyst. Mr. S. Lee speaks of large cysts being formed attached to the liver, omentum, and peritoneum. I have not seen any such case, but, on more than one occasion I have found the abdomen filled by hydatids, which presented many of the symptoms characteristic of ovarian disease. The *multilocular tumour*, having very many cysts varying very considerably in size and in the contents of each cyst, is the most frequent of all forms of ovarian disease, and the kind of case where tapping can be of no use, and where, if adhesions are not very extensive, extirpation is most likely to be successful. The size of such tumours as are likely to be brought forward for the prospects of an operation is generally large, twenty-five to thirty, or thirty-five pounds, is very common, and I have removed one successfully weighing seventy-three pounds. From these remarks, some idea may be formed as to the extent of material removed in one hundred and four operations, which could scarcely average less than thirty pounds each.

The character or general appearance of ovarian tumours is of some importance, as it is just probable that great uncertainty may exist in the diagnosis, and it may be necessary to make an exploratory incision. In such a case, after every other mode of inquiry has failed, the first appearance of even a very small portion of the tumour surface will decide if ovarian or uterine; the pink blush, and moderately even surface is the character of uterine tumour; whilst the whitish, muddy pearl-like appearance, with a bluish tint here and there, will never fail to characterize the ovarian mass, the surface is also more irregular, more lobulated; the vessels are large and flat in uterine tumours, and are scarcely discernible; in ovarian cases, they appear on the surface larger than they really are, being much flattened, and confined chiefly to the surface. In uterine enlargements the vessels are very numerous,

but more sub-divided and smaller, and interspersed throughout the whole mass. In all exploratory incisions, if the surface shows the pink shade before spoken of, the incision should be immediately closed. As to the interior of ovarian tumours, the cells or sacs vary considerably as to size, from that of a pea to the enormous sacs spoken of in the preceding remarks. The contents are equally various, some gelatinous, some thin, some contain pus of almost every form and consistence; in some of the large sacs hang pedunculated masses, like the main mass. I have not seen any hydatids in the interior of ovarian tumours.

The vessel supplying the pedicle will guide the opinion as to the vascular character of the whole mass; I have seldom found it larger than a crow-quill, although those on the surface appear much larger, and yet their supply must be derived from the vessel of the pedicle only.

Adhesions.—In many cases there is not the slightest adhesion beyond the pedicle, others are adherent, not only to the viscera, but also to the peritoneum, though not extensively; in both these classes I should operate without any hesitation; the character of the adhesions varies considerably, in respect to different organisms. Those to the intestines, are generally threads or bands of lymph, easily separated by the bistoury without hemorrhage, and are generally of no consequence. Those to the mesentery are patchlike, sometimes easily peeled off, but often firmly organized, and when separated are liable to hemorrhage; such separations should be watched, and if any tendency be shown to bleed, the vessel should be secured with fine silk, and the ends cut off close. Lastly, the adhesions to the peritoneum are in broad patches, and often peel off with moderate ease, but in some cases are so firmly organized that it is better to cut off the tumour at the edges of the adhesion, leaving the broad patch of the adhered portion of the tumour attached to the peritoneal surface, to slough off and discharge itself by the aperture left for the ligature at the pedicle. I have frequently done this without any bad consequences arising from it. It is a curious fact, that those cases have often made the most rapid recoveries where there have been the most extensive adhesions, and considerable force required to overcome them.

As the contents of ovarian cysts are very variable, it would occupy considerable time to describe each variety, and as it would not be any advantage either in treatment or operation to dwell upon this portion of our subject, I shall next make a few remarks as to the development of symptoms in this formidable disease.

The Symptoms of Ovarian Disease are very different when the disease occupies the abdominal cavity to what they are when only in the pelvic cavity in the earlier stages of its development. The *early symptoms* are deep-seated pains in the groins, bearing-down sensation in the vagina, a feeling of fullness, throbbing pain at the anus, more especially when voiding fæces, numbness on the side affected, loss of

motion, hemorrhoids, irregular menstruation, fluor albus, os uteri *in situ*, tenderness above the pubis inclining to one side; vaginal examination detects fullness on one side, with tenderness to the touch (these latter symptoms are more distinct on examination per rectum). constipation, flatulency, inability and desire to void urine, most of the symptoms simulating pregnancy. As the disease advances, and begins to occupy the abdominal cavity, many of the above symptoms are relieved, others aggravated; during the period of menstrual discharge very many of these symptoms are still further increased in severity.

Advanced symptoms.—Bladder somewhat displaced and more irritable, tympanites, sickness, occasionally œdema when the case is an extreme one, dyspnœa, often but not always fluctuation, belly shining, enlargement rather to one side, lobular, prominent at the centre, in extreme cases the ensiform cartilage and ribs forced upwards, umbrella form, if one or two cysts the fluctuation very distinct, if many cysts fluctuation obscure or altogether absent from the semi-solid character of the tumour; movements felt, often mistaken for fœtal, but only arising from the efforts of flatus passing along the intestines with difficulty, in consequence of the pressure of the tumour; the presence or absence of adhesion can only be attested by an experienced touch on the abdominal surface, and often great difficulties are encountered by the most experienced.

Examination per vaginam.—Vagina elongated, os uteri displaced, drawn upwards and laterally, the uterus free, natural in size, and easily moved about, unless the pelvic cavity is filled with tumour mass, in which case it is difficult to ascertain, as the uterus is often out of reach or flattened; the vaginal walls are pressed in on the diseased side.

Diagnosis.—From retroversion, it is distinguished by the situation of the os; from retroflexion, the uterine sound will easily determine; from ascites, by the system being in a healthier condition, by its lateral bearing, by duller fluctuation, by irregular surface, by elongated vagina, by os tilted on one side, and by seldom being accompanied by œdema of the legs. In cases where there is but one large cyst, with little or no solid mass, the distinction from ascites is more difficult, but such cases are rare.

From Pregnancy, by its lateral position, by regular or irregular menstruation, vaginal examination shows the uterus not enlarged, moveable, no fœtal pulsation; the latter, however, may be present in cases where pregnancy co-exists with ovarian disease.

From Cystic Tumours not Ovarian.—The history of the case; by the veering to one side, by the menstruation still less interfered with.

Enlarged Uterus.—Differs from ovarian, by being less lobulated, more central, easily detected by uterine sound, size and weight of uterus by vag. examination, suppressed menstruation, general health more disturbed, and complexion more sallow.

From distended Bladder.—By proper attention to symptoms, by which it will be impossible to confound it with ovarian disease.

Accumulated flatus has been mistaken for ovarian disease, and operated upon for it; in the present state of our experience such an accident seems impossible.

Other enlarged Viscera.—An enlarged liver or spleen might possibly be mistaken for ovarian disease, but it is not very probable. As a rule, with enlarged viscera, the lower third of the abdomen is pretty free from tumefaction, whilst in ovarian disease that lower third is filled in preference. This point, coupled with the prominent constitutional disturbance in visceral enlargements compared with the trifling disturbance in ovarian mischief, will generally be a sufficient distinction.

With respect to the *Treatment of ovarian disease* when once fairly and extensively developed, I have so little faith in any benefit by medicine, that I shall not repeat what I have already stated on that question in my 'Manual of Obstetric Operative Surgery.' I have not found any remedy that can in the least be depended upon in the form of medicine. *Tapping*, as a curative measure, is the worst that can be attempted, and can only be justified in cases where the operation prospects are hopeless, or where the invalid is determined not to submit to its chances. Tapping at all times can only be advanced as a means of temporary relief, and as a rule, only spurs the secretory sacs to refill more rapidly.

Injecting with tinct. of iodine has succeeded in my hands frequently, but can only be justified where there is but one sac, where the walls are thin, and where there is no nucleus of solid tumour; lastly, in apparently the most favourable cases it has frequently failed.

Extirpation, where it can be done, is the only practical remedy, and is one in which, if it succeeds, the cure is permanent. By extirpation, I mean by the large incision, that is *as large* as is necessary for the operator to manipulate easily and rapidly for effecting its removal. My friend, Dr. T. Smith, of London, considers the exposure of viscera in the operation for a length of time as a great source of danger. I fully agree with him on this point, and it is on that account I advocate a free and large incision, believing when the operator has room to manipulate freely, he lessens the time of exposure very considerably, at which time, if the temperature of the room is carefully attended to, the danger cannot be increased.

I shall now conclude with a few statistical remarks on this operation. My own mind is still impressed with the importance of taking, as a guide to this question, the experience of those only, who have, like myself, operated a sufficient number of times to have formed some definite ideas on the points necessary to be enforced to secure the best prospective results, rather than to rely on general statistics which must of necessity be built up by a number of isolated cases by individuals who have scarcely ever operated a second time, or at most some two or three cases, both of which classes of individuals cannot have met with much to enhance their own experience sufficiently to enable them to

become safe guides for others of still less experience to follow. To mix the results of these limited operators with the whole has a tendency to depreciate the general statistics materially, and lead to wrong impressions. This is the principal error in the lists of cases collected by Drs. Robert Lee and Safford Lee, as also in the very excellent and elaborate table of ovarian operations by John Clay, Esq., of Birmingham. I prefer statistics drawn from those operators who have performed it frequently, men who have had frequent opportunities of testing their practice, and from these alone I would adduce the best rules for the guidance of other operators in future. It must be apparent to all on reflection that other information, however successful, must be so perfectly haphazard or accidental that such should not be mixed up with the general question. Indeed, such is often the unlooked for result of first cases undertaken without previous experience, that I am not certain if it would not benefit the general question more if the first three or four cases of every operator were rejected, as it is well authenticated that they will often recover under the most extraordinary disadvantages, whilst, on the contrary, they may sink without an apparent cause. In the 'Dublin Quarterly Journal' of Feb. 1861, is reported a case of ovarian extirpation *with successful results.* In the report are some very lengthy instructions how to conduct such cases to a favourable issue, although it was the author's first and only operation, and, singularly enough, the case *never did recover*, but died almost as soon as the report of her cure was announced to the public, at least early in the March following, the date of the case in the journal being February. Since 1842 I have operated on 104 cases, 97 of which have been under my own treatment throughout. The remaining seven were operated on at a distance from home, and the care of the cases was left to others, assisted by any suggestions I could make from time to time, but not accepting the whole responsibility. The following figures show the general result up to the date of this communication :—

Cases.		Deaths.		Recoveries.
97	...	29	...	68
7	...	3	...	4
Total..... 104		32		72

Taking the odd figures aside, the recoveries in round numbers will be as 70 per cent. From the length of time the disease has been established, when the case is presented for operation, the great debility of the system from frequent tappings, or the secretory supply called into action, I do not think we can reasonably hope to extend our success beyond 70 per cent.; but even this far exceeds the first expectations, and is sufficient to establish the operation as a legitimate one in all well-selected cases, in the minds of those who are desirous of extending the benefits of surgical skill to these unfortunate sufferers. —*London Med. Review, Aug. and Nov.*, 1861, *pp.* 59, 205.

126.—ON OVARIOTOMY.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[Mr. Wells relates several cases of ovariotomy. The last is the most important. The patient died twenty-four hours after the operation. The case was a very unfavourable one, the cyst being a large compound one, having many adhesions. The peritoneal cavity contained from four to five pints of reddish serum.]

The serum found in the peritoneal cavity must have been a very active animal poison, for I suffered severely two days after the examination from a very slight scratch with the point of a needle on the left forefinger. I sucked the spot instantly, but the next day a small vesicle formed, and I applied caustic freely. On the second day I had severe rigors, lasting several hours, with intense headache, relieved by vomiting and a copious perspiration, which lasted about eighteen hours. For several days afterwards I was very weak, but all the severe symptoms had passed off by the fifth day after the puncture.

This recalls a question I have raised before. The peritoneum contained some pints of poisonous serum. It was probably formed by part of the membrane, and might be absorbed by other parts. If so, a poison which affected me so severely in a small dose might easily kill any one in a larger dose. I recovered after the absorption of a fraction of a drop; but the poor woman was overpowered by the quantity taken up by her own absorbents. I am quite aware of the objection some may offer, that a fluid poisonous after death might not have been a poison during the life of the woman; the reply is, that twenty-four hours was too short a period after death for decomposition to have occurred; and also, that all experience has proved inoculation of the fluids of those dying of puerperal peritonitis, erysipelas, &c., to be far more dangerous when the bodies have been fresh, than when putrefaction has commenced. This is not a mere question of theory or curiosity, for it leads to an important rule in practice—or, rather, to the suggestion, that in cases where such poisonous serum may reasonably be supposed to be present, and *à fortiori* when there are physical signs of its presence, that a part of the wound should be opened to allow of the free escape of the serum by the side of the peduncle. In two cases I formerly published I acted upon this rule with the greatest advantage, and both patients recovered. The question also arises, whether one is not apt to unite the wound too closely round the peduncle. The fear is that peritonitis may be set up by leaving *any* opening; and one generally closes the wound carefully around the peduncle, partly to prevent entrance of air, and partly to prevent gravitation into the abdomen of the putrid fluid formed around the stump during the time the ligature is in process of separation. This plan may be a good one on the whole; but I am disposed to think that in many cases where there is such a condition as I have

described after operation, a free opening should be made for the escape of the serum.

Another question, of even greater importance, is suggested by this case—Is it right to perform such an operation as ovariotomy in unfavourable cases? It may be said that by doing so the surgeon not only risks his own reputation, but lowers the operation he performs in the estimation of the profession, and thus lessens the number of favourable cases who might be willing to undergo it, were it not known that one in two, three, or four who do submit to it, die. It is quite clear that a surgeon who will only operate on very favourable cases ought to show far better returns than one who consents to stake his own reputation in order to give a dying patient a small chance of recovery; and it may possibly be right to follow the more prudent course. But in a case where a poor woman says, as many have said to me, "I suffer from a disease which must kill me. I cannot live very long. My life must be a life of suffering. If you operate, I know the risk I run; but I *may* be cured and return to my husband and children, and I would rather die than live as I am,"—in such a case as this, I do not envy the feelings of a man who—unless he saw the case was absolutely hopeless—would let any consideration for the general character of surgery, or for his own reputation as a successful operator, induce him to refuse the prayer of the poor dying creature who placed her life in his hands.

Still, I think it is right that some classification of cases should be adopted, for what we want in practice is not an answer to the question "What is the general success of ovariotomy?" What the patient and the surgeon want to know is, "What are the probabilities of success or failure in this particular case?" and this knowledge can only be gained by testing the operation in various classes of cases—distinguishing those patients who are young and healthy from those who are old, or worn out by frequent tappings or long-continued disease, and so on. If I were to exclude the fatal cases in my own practice which were regarded by myself and others in consultation as unfavourable before operation, I might say that I had scarcely lost a single case; and my experience of those cases which may be regarded as favourable is bringing me to the conclusion that ovariotomy is one of the most successful of our capital operations, and not very much more dangerous then a simple tapping.

Taking favourable and unfavourable cases together, the following is the general result of my experience of the operation in hospital and private practice:—

	Cases.	Deaths.	Recoveries.
Hospital	14 ...	5 ...	9
Private	10 ...	3 ...	7
	24	8	16

I need hardly say that amputation of the thigh, ligature of the

larger arteries, lithotomy in the adult, and other capital operations called "legitimate," show a general result less favourable than this operation, which is still stigmatised by some writers as "unjustifiable." —*Med. Times and Gazette, May* 25, 1861, *p.* 546.

127.—HYSTERIA MASKING SECONDARY SYPHILIS.

(Case under the care of W. Coulson, Esq., St. Mary's Hospital.)

[There are cases in which the hysterical tendency is evinced less by the simulation of disease which does not exist, than by the exaggeration of symptoms of some affection which has an actual existence. In such a case is it better to treat the case from the hysterical point of view, or to strike at the malady upon which the hysteria is superadded? We believe that in practice the latter will be found to be the most successful treatment.]

This patient, a young woman aged twenty, when admitted, complained of stiffness of the elbow-joint, which was retained in a position midway between flexion and extension, but could neither be flexed nor extended. There was a history of a blow on the elbow some three months previously, but without applying the test of anæsthetization by chloroform, Mr. Coulson was able to conclude that there was no organic local cause for this affection, and that it was mainly of hysterical origin. Further inquiries, however, into the history of symptoms showed a suspicious condition of the throat, and some tenderness and nodular roughness along the shin bones. These were symptoms indicative of a secondary taint. Mr. Coulson ordered three grains of iodide of potassium in an ounce of infusion of quassia three times a day, and two grains each of blue pill and Dover's powder at night. The secondary symptoms yielded to this treatment, and the hysterical affection disappeared simultaneously. The patient was discharged at the end of three weeks, having the free use of her arm, and feeling no pain in it.—*Lancet, May* 18, 1861, *p.* 483.

128.—ON IRRITATION OF THE URETHRA IN FEMALES.

By John Hunter, Esq., M.A., Manchester.

[The following is one of a number of cases which the writer has lately had under his care, not particularly described in books, and not perhaps fully recognised. The age of the case must not be taken as an example of the ages of the others, for the third case related is only twenty-two.]

Mrs. C., aged seventy-two years, consulted me in the summer of 1859, whose case presented the following features:—During the past fourteen years she had suffered from incontinence of urine—in fact "she was never dry." Together with this there was a constant desire

to void her urine, but instead of each attempt to do so bringing relief, it only caused an increase of her sufferings; yet still the desire was persistent. Walking or standing greatly aggravated her sufferings. As much of her urine as could be collected was much clouded with mucus; there was pain and a feeling of weight in the lumbar region. The treatment hitherto had consisted in lotions and other applications to the external parts and to the vagina, but without bringing any relief. On examination, the external parts appeared and felt to be quite natural, but the urethra, from the meatus up, as far as could be discerned, was very red and congested, quite as much so as the vascular tumours sometimes seen near the meatus. On the introduction of a silver catheter, the whole urethra was found to be exquisitely tender, more particularly so at about the external third. The bladder itself did not appear to be affected; in fact, the disease seemed altogether confined to the urethra. The urine, on examination, was found to be not more acid than natural.

The treatment I adopted consisted in the exhibition of alteratives, with the constant use of small doses of liquor potassæ. The urethra itself I treated locally in the following manner: I first injected a little warm water from a male glass syringe, in order to wash away the adherent mucus, taking care that the nozzle of the syringe was fully introduced (with a slight rifling movement) and that it did not impinge on the walls of the urethra. I then threw up a syringeful of solution of nitrate of silver (five grains to the ounce), and allowed it to come away of itself. I enjoined her to rest in the recumbent posture, and as far as possible to resist the desire to empty the bladder. I used the same local means every day, and gradually the mucous membrane of the urethra appeared to be less irritable, and, as a consequence, the contents of the bladder were retained for a longer time; till, in about a fortnight from the commencement of the treatment, the patient stated that she was able to hold her urine for about four hours, the pain had almost disappeared, and the general condition of the patient was satisfactory. I have since attended her for another disease, and learned with satisfaction that she had no return of the urinary derangement.—*Lancet, May* 18, 1861, *p.* 480.

129.—EXPLANATION OF HYSTERICAL PAINS IN THE HIP OR KNEE-JOINT.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital, &c.

The sacral ganglia and the lower lumbar ganglia of the sympathetic nerve are connected with the great sciatic nerve, and in part with the obturator nerve. These same ganglia are connected likewise with the nerves proceeding through the broad ligament to the uterus and to the ovaries. I think we have here, then, an explanation of the frequent occurrence of what we call hysterical hip-joint or hysterical

knee-joint. If the nerves in the ovaries or the uterus be in a state of irritation, that irritation can be conducted to these sacral nerves or to the obturator, and then, in accordance with the generally received law of distribution of nervous influence, irritation or pain may be manifested at the other peripheral or articular end of the same nerve; so that it may be expressed within the knee-joint, on the inner side of the knee-joint, or it may be within the hip-joint, because the hip as well as the knee-joint receives its nerves from these various sources. You will observe the posterior part of the hip-joint and the nerves coming to it from the sacral plexus. Part of the sacral plexus receives its filaments from the sacral ganglia; so do the uterine and ovarian nerves; and it is quite possible, nay, I think it is very likely, that the irritation commencing in the ovaries or the uterus might be conveyed to some of those filaments derived from the same ganglia in the sacrum, and irritation in the hip-joint be thus produced. You will observe that the two nerves, which are so distributed as to be capable of producing the pain in the joints, are the great sciatic and the obturator; and I believe this is a probable explanation of the fact, that of all the joints in the human body affected, as we term it, hysterically, none are so frequently involved as the hip and the knee-joints. By tracing these two nerves, I think we may see a probable interpretation of that frequency.

We know that the spinal nerves communicate with the ganglia; they go on through the ganglia and join the filaments of the sympathethic—travelling upon the arteries, for example—and are thus transported to the intestine, uterus, or ovaries. Let us assume, then, that a patient may have irritation from any cause in the intestine, in the uterus or ovaries, or in the broad ligaments. Thus, we may trace the course of that intestinal, uterine, or ovarian irritation through the ganglia, through the spinal nerve and spinal marrow, thence to be reflected to any part of the peripheral or articular distribution of that same spinal nerve. This condition, I apprehend, is very clearly recognised sometimes in the case of the intestines. Who is there that has not felt the irritating pains and gripes in the interior of the intestines from some morbid agent lying there, or from drastic purgatives traversing the gut, and at the same time pains or cramps in the leg and pains in the loins?—conveyed in the latter instance by the filaments of spinal nerves, which pass to the posterior part of the body or the lumbar region. And is it not a common occurrence in cases of uterine and ovarian irritations for the patients to complain of pain in the loins, but particularly over the posterior part of the sacrum? The ovarian and uterine nerves traverse the ganglia of the sympathetic, and so reach the spinal nerves, and hence the morbid influence conveyed by the posterior branches of the spinal nerves to the skin over the lumbar and sacral regions, explaining the lumbar and sacral pains experienced by such patients.—*Lancet, Sept.* 14, 1861, p. 247.

130.—ON LEUCORRHŒA AND VAGINITIS.

By Dr. Charles Waller, Obstetric Physician to St. Thomas's Hospital.

[The subject of leucorrhœa is almost co-extensive with diseases of the generative organs, as there are but few affections of these parts unattended with leucorrhœal discharge.]

Leucorrhœa is sometimes the result of debility and relaxation: it is, however, not unfrequently the result of the opposite condition of parts, where there is active inflammation of the vaginal membrane, partial or more general; this has sometimes been termed "inflammatory leucorrhœa." Vaginitis is, however, the more appropriate appellation, as this is descriptive of the *disease*, whilst the other merely enunciates a symptom. The discharge sometimes proceeds from the uterine membrane, either entirely, or in addition to the vaginal flow; this has been termed uterine or utero-vaginal leucorrhœa. There is yet another form, first described by the late Sir Charles Clarke, and named by him "white, opaque mucous discharge," and he thus accurately describes it:—"This discharge is opaque, of a perfectly white colour, and it resembles in consistence a mixture of starch and water made without heat, or thin cream; it is easily washed from the fingers after an examination, and is capable of being diffused through water, rendering it turbid. A morbid state of the glands of the cervix of the uterus probably gives rise to this discharge; at least, the cases in which it comes away are those in which the symptoms are referred to that part; and when pressure is made upon it, the woman experiences considerable pain." And again, "the discharge of which the above definition is given belongs to one morbid state of the uterus only; but it characterises that state with marked constancy." Remember, lastly, that the malignant diseases of the womb are never unattended with leucorrhœal discharge: in these latter cases there is always more or less blood mingled with it.

Vaginitis.—There are two distinct varieties of this disease. In the one, the symptoms are those of acute inflammation; in the other, the membrane is in a state of passive congestion. In other words, there is active and there is passive vaginitis: let me now request your attention to the former variety. If vaginitis be of the active kind, and extend over a large portion of the canal, the symptoms are too characteristic to be mistaken for any other disease: the vascular excitement and irritation are very distinctly marked, and in the more severe forms constitutional derangement will be added to the local symptoms; pain is felt in the vaginal passage, varying in degree and character: it is sometimes described as darting and throbbing; at others, as hot, scalding, or smarting, closely resembling the pain felt at the extremity of the rectum during the action of some powerful drastic purgative. The inflammation often creeps along the membrane of the urethra, and then, in addition to the symptoms enumerated,

there will be smarting and cutting pain whenever the patient passes water *(ardor urinæ)*. It does not always stop here, but extends into the bladder itself, causing frequent and sudden desire to micturate, with considerable pain in accomplishing the act. The discharge does not consist in a mere increase of the natural secretion of the part, which is of a bland, unirritating quality, but becomes entirely changed, even in its appearance; it is copious but much thinner in consistence than ordinary, and very irritating in its character, often producing inflammation and sometimes excoriation of the skin outside the genital fissure, where there has not been sufficient attention to cleanliness. A very foul ulcerated surface is sometimes produced by this cause.—There is another symptom often if not generally present in acute vaginitis which is very distressing to the patient; namely, a constant and painful sensation of "bearing" or "forcing down," probably inducing suspicion of prolapsus uteri. On examination, you detect no displacement of the organ; and on inquiry, you find that the recumbent position does not relieve the uneasiness. From what cause does this arise? Evidently from the inflammatory state of the upper portion of the vaginal membrane, which cannot then bear the pressure of that portion of the cervix uteri which projects into it, although insensible to it in the natural condition of the parts. If a digital examination be made, pain will be experienced: the extent of such pain, whether partial or more general, will enable you to determine with tolerable accuracy the extent of the inflamed surface; should the speculum be employed, the membrane will appear turgid with blood, either of a bright red or vermilion hue, or a more dusky crimson colour, according as the disease is more or less active in its character. I have already told you that in unmarried females the speculum cannot be easily introduced; nor is it necessary in the vast majority of instances, as the probability is great that simple inflammation exists. In those who have borne children, you often discover some structural change around the os uteri, in cases of protracted leucorrhœa, which will, of course, require special treatment. After long-continued leucorrhœa, changes occur in the vaginal membrane: There are, sometimes a number of slight excoriations along its entire surface, or confined to the portion reflected over the cervix uteri; or there may be distinct elevations without ulceration, producing that peculiar appearance which when occurring in the eye is called "granular conjunctivitis." This inflammation may proceed still further, and produce warty excrescences, few or many, soft in their texture, tender to the touch, secreting an irritating fluid which, if not constantly washed away, produces an extreme degree of heat and smarting, which greatly aggravates the sufferings of the patient.

These symptoms, you are aware, may be traced in many instances to gonorrhœal infection; but as they may and do occur as the result of simple acute vaginitis, it behoves the practitioner to be very careful, and not hastily to pronounce an opinion which might for ever damage

the reputation of an innocent female, or, it may be, mar the happiness of domestic life by raising injurious and unjust suspicions of the conjugal fidelity of the wife or husband. So virulent is this discharge, that it will occasionally produce great irritation in the male organ if exposed to its influence: this may be either in the form of urethritis with muco-purulent discharge, or it may even cause ulceration. I once attended a gentleman who was suffering from a slight but irritable ulcer on the prepuce, clearly traceable to this cause; there was not the slightest reason to suspect a venereal taint, and the rapidity of its cure under very simple applications convinced me of the correctness of my opinion. This gentleman, when I first saw him, was in a considerable state of excitement; he could not but feel suspicious as to the cause of his malady: he soon calmed down, however, on having the nature of the case fully explained to him.

Treatment of Vaginitis.—The treatment of vaginitis must necessarily depend upon its character, whether active or passive—its extent, whether partial or general, and the state of the constitution in which it occurs—whether the patient be robust and plethoric, or in the opposite condition of debility and exhaustion.

Let us first take a case of recent acute vaginitis, with a constitution unimpaired, no debilitating effect having as yet been produced. Although inflammation of the mucous membranes of the body seldom if ever require the powerful mode of treatment adopted for the cure of inflammation when occurring in the serous membranes, you will nevertheless find it necessary in many cases to adopt mildly antiphlogistic measures. especially where the disease is extensive enough to produce general vascular excitement unattended with symptoms of depression. I need hardly tell you that the arrest of the flow is not the primary indication; for if this were suddenly suppressed, the natural result would be an increased engorgement of the vessels, which were to a certain extent relieving themselves by increased secretion. Means must be had recourse to for the purpose of subduing the intensity of the inflammation; and if a favourable effect be produced by these means, the leucorrhœa will in all probability be increased in quantity and altered in quality: instead of remaining thin, ichorous, and irritating, it becomes more viscid and bland. In the severer forms of vaginitis, there is very often a little blood mixed with the vaginal discharge, so that the flow is streaked with blood. It is, as I have said, seldom necessary to make use of very powerfully depressing remedies for the subjugation of this form of inflammatory affection. The first thing to be done is to enjoin the recumbent position; and to this the patient must strictly and resolutely adhere; the cure will be greatly protracted without strict obedience to this injunction. In cases now under consideration, where the inflammation is acute, and the system in a plethoric state, marked by a hot skin, strong pulse, furred tongue, &c., a question as to venesection may probably arise: I say *possibly*, because it has been recommended by some;

I have never seen a case in my own practice where I have considered general blood-letting to be either necessary or advisable. Great benefit is derived from the application of four or six leeches, just within the ostium vaginæ. This part is very vascular, and bleeds freely. The oozing from the leech-bites should be promoted by long-continued applications of warmth and moisture, either from the use of fomentations of warm water, or from the frequently-renewed applications of wetted spongeo-piline; the latter is in most cases preferable, as it preserves the bed-linen from being soiled or wetted. The bleeding orifices should be sponged occasionally, to wipe away any coagulum that may form and obstruct the flow of blood. In the early stage of acute vaginitis, relief is speedily obtained; the heat and pain subside, the sense of fulness diminishes, and the febrile symptoms lessen. The vagina should be frequently washed out with warm decoction of poppies, used as an injection. This acts beneficially in a twofold manner: in the first place it clears away the discharge, which has an irritating effect on the parts with which it comes in contact; and secondly, by acting as a fomentation on the affected parts, it assists in lessening inflammatory action. The necessity of frequent ablution, in some form or other, was insisted on by Heberden, whose words I now quote: "Humor iste, quanquam plerumque albus, ut vulgo appellatur, et aquæ similis, interdum tamen glutinosus est, et coloris subflavi, item subviridis et mali odoris, atque tam acris ut nisi partes in quas descendit sæpe eluanter, levis inflammatio fiat cum prurigine et dolore: cuticula quoque deteratur, et urina reddi nequeat nisi cum sensu pungentis caloris." In some few instances the application of cold is more agreeable to the sensations of the patient; and then the liquid should not be warmed before being used. Other forms of injectiom may be used, as the following:—

℞. Ext. conii, ʒiij; aq. destill, ℥xij. Solve.

Or, ℞. Ext. belladonna, ʒij.; aq. destill., ℥xij. Solve.

These may be continued or changed as circumstances require.

Mild laxatives are required in the early stage; and these should be repeated for a longer or shorter period of time, according to the urgency of the case. Drastic purgatives are to be avoided, as they are likely to increase the irritation already existing. The nitrate of potash combined with a saline aperient has often a beneficial effect. The following is my usual form of prescription:—

℞. Potas. sulphat., ʒj.; potas. nitrat., ʒj.; aq. menth. pip., ℥iv.
M. et capiat ter die partem tertiam.

The diet must be spare and of an unstimulating quality, no fermented liquors being allowed until the inflammation has been subdued. This plan should be strictly pursued for a few days; and should there at that time be no relief—if the pain, &c., continue, leeches must again be applied, the potash mixture continued; and if the nights be restless and sleepless, an anodyne should be administered at night: half a grain of opium with one grain of chloride of mercury is generally

sufficient to effect your purpose. It is seldom necessary to proceed further with the bleeding, and, indeed, it often happens that the second application is not required; the symptoms are alleviated, the uneasy feelings subside; the discharge may not be diminished in quantity, but it improves in appearance. Do not, however, discontinue the laxative altogether; let it be taken twice instead of three times in the day. In the more favourable cases this is all the treatment required: perfect rest; the application of leeches where necessary; the administration of laxative medicines; spare diet; and frequent ablution of the parts. The disease ends in resolution; that is to say, the inflammatory action ceases, and the membrane at once resumes its ordinary functions. Not so always; for after the active symptoms disappear, the disease perpetuates itself in the more chronic form: although there is to a great extent relief from pain, the discharge continues profuse, though not irritating; it becomes thick and glutinous, the patient's strength begins to yield, her vital powers diminish, and her spirits are depressed. If the parts be examined at this time, the membrane will be seen distended with blood, the colour being much darker than in the active form of the disease; little or no pain is produced by the examination. These cases are often very tedious and difficult of cure. Slightly astringent injections should be employed; *e.g.*,

℞. Plumbi diacetat., gr. xxiv.; aq. destillat., ℥xij. Ft. lotio.

The general plan of treatment ought to be moderately tonic, and the recumbent position still adhered to. After a week or ten days the injection may be changed for the following:—

℞. Argent. nitrat., gr. xxiv. ad xxxvi.; aq. destill., ℥xij. Ft. lotio.

The progress to convalescence will very much depend upon the patient's state of health; if this be seriously affected it will be long before the patient is restored to health, and a long course of tonic medicine will be required, together with the usual adjuvants for invigorating the powers of the system; namely, well-regulated generous diet, a due proportion of wine, and a moderate amount of exercise, taking care that this never amounts to fatigue.—*Med. Circular, Jan.* 9, 1861, *p.* 19

131.—ELECTRICITY IN AMENORRHŒA.

By Dr. Althaus.

I have found electricity externally applied a very valuable emmenagogue in young and unmarried women, in whom we are led to assume a torpid state of the vasomotor nerves of the ovaries and the uterus, and also in such cases where the catamenia have been suppressed in consequence of emotion or cold. Faradisation has proved especially useful; I have also employed the continuous current, but with less benefit; while I cannot speak from personal experience on the value in these cases of frictional electricity, which I consider an antiquated remedy, and which should no longer be employed in practice.

I am well aware that in many cases of amenorrhœa the application of electricity has been followed by negative results; and I am quite ready to admit that electricity, even when properly applied, is not infallible in the class of cases I have alluded to. But I consider that very often the method of application has been such as to render a beneficial result all but impossible. Only recently, a case has come under my notice in which a medical man believed himself to be engaged in applying the Faradic current to a woman suffering from amenorrhœa. On inspection of his manner of administering the current, however, it was discovered that he had united the conducting wires with the poles of the cell instead of with those of the coil! As the apparatus he had on the table was a powerful one, he was greatly astonished that his patient had not the slightest sensation of a current passing, and he concluded that it was a case of deep anæsthesia of the genitals! His astonishment increased when, the wires being put into their proper places, the anæsthesia suddenly disappeared, and the patient called out that she was now fully sensible of the passage of the current! Although cases of such gross ignorance may be rare, still there is no doubt that electricity is only seldom applied as it ought to be.

Another mistake is frequently committed which prevents beneficial results taking place. Doctors as well as patients are disappointed if no immediate benefit results from the employment of a "wonderful remedy," as electricity is unfortunately called. But perseverance is just as necessary with the electric treatment as it is with many internal remedies; and if perseveringly carried out, this plan of treating obstinate cases of amenorrhœa will be found to be attended with the best results, although signs of an improvement may be wanting for some time. In one patient whom I had recently under my care, the catamenia only appeared after the forty-sixth *séance*, when all hopes had been well-nigh given up by her; and it is only in a proportionately small number of cases that electricity yields rapid curvative effects.

As to the parts of the body to which the current should be directed, I may add that Faradisation of the skin of the soles of the feet by means of wire brushes, or Faradisation of the abdominal parietes by means of moistened conductors, and finally the application of one moistened conductor to the nape of the neck and of another over the os pubis, are the most effectual methods in cases of this kind.—*Med. Times and Gazette, June* 22, 1861, *p.* 662.

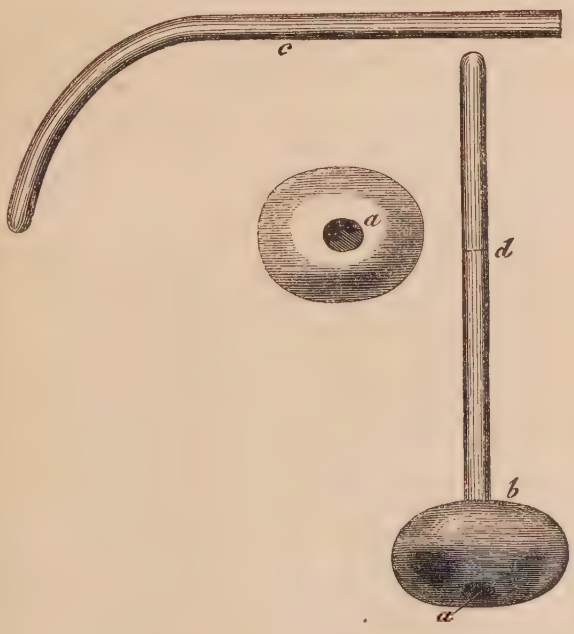
132.—ON THE LOCAL TREATMENT OF AMENORRHŒA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[It often happens in cases where the constitutional causes of amenorrhœa are removed, that still the amenorrhœa persists, as if some local action or irritation required to be superadded in order to initiate

the menstrual action. Under the term "local treatment" are comprehended various local emmenagogues applied to the vagina, and to the uterus. Direct stimulants applied to the uterus or uterine cavity, are undoubtedly the best means at our command for recalling the suspended menstrual function. The best mode of applying a direct stimulant to the uterine cavity is the introduction and wearing of a small and usually galvanic intra-uterine pessary, respecting which Dr. Simpson makes the following observations.]

Intra-uterine Pessaries.—This form of instrument consists simply, as you perceive, of a small copper bulb (see Fig. *a*, *b*), from the middle of which there rises a stem *d*, made half of copper and half of zinc, and measuring $2\frac{1}{3}$ inches in length, or rather less than the length of



Full-sized sketch of the galvanic intra-uterine pessary, *a b d*, and of the staff, *c*, used for introducing it. *a*. Perforation on the lower surface of the bulb, *a b*, of the pessary, into which the point of the staff is made to fit: *b d*, stem, or intra-uterine portion of the instrument, consisting in the lower half, from *b* to *d*, of copper, and towards the point, of zinc.

the uterine cavity. In introducing it, the patient is placed as usual on her left side, and the forefinger of the right hand having been brought into contact with the os uteri, the instrument is guided along it either with the right thumb or with the fingers of the left hand until the point of it passes through the orifice into the canal of the cervix. Usually you will experience no great difficulty in passing the instrument in this way up as far as the os internum; and if you now remove the point of the forefinger from the os and apply it below the bulb of the instrument so as to press it gently upwards, you will sometimes succeed, with the greatest ease, in insinuating it entirely into the uterine cavity, till the upper surface of the bulb is felt to be in apposition with the lips of the cervix. In other cases, however, you will meet at this stage with some degree of difficulty, for the os internum is occasionally the site of a kind of stricture, and this state of matters is further not unfrequently complicated by a slight anteflexion of the whole organ at the union of the body and cervix, so that the point of the pessary is apt to be caught at this level, and its complete entrance into the uterine cavity obstructed. In such cases you will best effect your object by pressing the bulb of the bougie very much towards the hollow of the sacrum, so as to tilt forwards the point of it, and to make it move slightly from side to side, or from before backwards, when you will feel it suddenly passing the constricted spot, and the uterus settling down, as it were, on the instrument, without the exertion on your part of any active or injurious amount of upward pressure. I have spoken of the introduction of the intra-uterine stem-pessary as being effected by the unaided action of fingers, because, for a long time past, I have never had recourse to any other means. But, as you may observe, there is, on the surface of the bulb opposed to that from which the stem springs, a small orifice, *a*, intended to admit the point of a staff, *c*, which may be used to facilitate the introduction of the pessary. The staff is made of steel, is about eight inches in length, and is slightly bent at the point, so that when it is fitted into the hole in the bulb of the bougie, the latter can be guided by means of it in any direction, with more precision and power. I fear, however, that no mere description I can give you as to the mode of passing the intra-uterine pessary will enable you in any case to effect its introduction. This is one of those little operations which you will only succeed in performing properly by a certain amount of practice; and I believe that after you have tried it a few times, so as to have gained a little tact and skill in the manipulation of the bougie, you will come to effect its introduction easily with the fingers alone, and discard altogether the use of the staff. There is one secret regarding the introduction of this instrument that I have to tell you. Sometimes you may have succeeded in passing the uterine sound easily enough, but are foiled in your effort to introduce the permanent pessary. In such a case you may save pain to your patient and trouble to yourself by re-introducing the sound, and

leaving the patient lying with it in the uterus for half an hour or an hour, at the end of which time you will find it is so placed or so patulous as readily to admit the intra-uterine bougie. But you may ask, will the instrument thus introduced remain of itself *in situ?* In the majority of cases it will; and always with most certainty in those where the internal orifice has been so contracted or curved as to render the introduction of it somewhat difficult. In other cases its re-introduction is attended with no difficulty or discomfort, and you will find that after it has been worn for some days, the zinc half of the stem becomes covered with a white saline crust of greater or less thickness, which tends to enable it afterwards to keep its position. Though the zinc portion is generally deeply encrusted on the withdrawal of the instrument, the copper portion is always, on the contrary, clean and clear of all deposit. Before re-introducing it, remove the crust from the zinc portion, and wash it with vinegar. If you find it necessary you may sometimes introduce below the instrument, as I have not unfrequently done, a small gutta-percha pessary, such as we use in the treatment of prolapsus uteri, which will support the intra-uterine pessary and prevent it from slipping out. I never saw the use of this instrument for the cure of amenorrhœa attended with any untoward result, and I and my assistants have employed it in a very great number of cases. Often we see its introduction immediately followed by the most marked results, especially with regard to the cure of the headaches and other secondary symptoms; and if worn for a sufficient length of time, it does not often fail in exciting the uterus to the performance of its functions; or in recalling them when they have become suspended. I have seen menstruation speedily recalled by it after almost shop-loads of physic had been used in vain for the purpose. It produces its effect, I believe, simply by the gentle and continued stimulation of the whole uterine system. Probably other kinds of foreign body lodged there would produce much the same effect; and I have treated cases of amenorrhœa with success by the introduction of a stem pessary made entirely of copper or of German silver, such as is used for dilating the cervical canal in cases of dysmenorrhœa. But in cases of simple amenorrhœa I prefer to have the instrument made of two different metals, as suggested by Dr. Weir, because the slow galvanic action resulting from the chemical changes effected in the zinc increases the chances of a speedy and successful issue. For, let me remark, finally, there is yet one other means which has often been much relied on and bepraised as eminently serviceable for the cure of amenorrhœa, viz., the application to the uterus of

Electricity and Galvanism.—Among their multifarious medical applications, galvanism and electricity have been from time to time employed in the treatment of various uterine disorders; and in the latest American work on their use in medicine, by Dr. Garratt, of Boston, I find the author speaking of them as almost the only certain

means of exciting the action of the uterus in cases of amenorrhœa. To apply them to this organ you need to be provided with special forms of conductors. I believe that the slow but long-continued action of the intra-uterine galvanic pessary will produce the effect with far more certainty, than the repeated application of a stronger current; and I am sure that when you have found how easy it is, after a little practice, to introduce the instrument, and when you have witnessed in a few cases the safety and success that attend its employment, you will become more and more inclined to treat those very obstinate cases of amenorrhœa by means of the galvanic bougie rather than by any more irksome or prolonged plan of treatment. The galvanic pessary, however, is a measure which you would not be justified in ever employing in young and unmarried patients, except the great obstinacy of the case, the severity of the sympathetic or secondary effects, or the danger to the patient's health and life, gave you adequate warrant for such interference. In the less claimant cases of amenorrhœa we would rejoice to find any external mode of applying electricity effectual as an emmenagogue, whether the electricity were derived from induction, as when the so-called "Faradaic currents" are used; or from chemical decomposition, as in the various forms of galvanic battery; or from friction, as when electric sparks and discharges from the Leyden jar are employed. Of the effects of Faradisation in amenorrhœa I know little practically. But I have seen the galvanic current employed some fifteen or twenty years ago often, and certainly, on the whole, with little or no emmenagogue effect. About a quarter of a century ago electric sparks directed through the uterine region were tried in many cases of amenorrhœa by my predecessor, Dr. Hamilton, and my friend Dr. Ziegler, who is most thoroughly acquainted with medical electricity and its applications in practice. I have Dr. Ziegler's high authority for stating that, after making many trials of it in amenorrhœa the impression left on his mind is that, as an emmenagogue, it is in the main quite ineffectual and useless. It is but right, however, to add that some observers, as Dr. Golding Bird, have arrived at a different and far more favourable conclusion, but I am inclined to place great reliance upon the results of Dr. Ziegler's experience on this subject.—*Med. Times and Gazette, June* 15, 1861, *p.* 622.

133.—CASE OF DYSMENORRHŒAL MEMBRANE.

By Dr. Beatty.

At a meeting of the Pathological Society of Dublin, Dr. Beatty presented a specimen of membrane formed and thrown off during difficult menstruation by a young married female. He observed, that from the peculiar character and appearance of membranous growths of this kind identical in structural aspect with "decidua vera," and their being formed and thrown off during the paroxysms of dysmenorrhœa

by unmarried females, they sometimes gave rise to painful suspicions and mental disquietude on the part of the patient's friends and relatives. These adventitious growths, peculiar to dysmenorrhœa, are found to occur in maidens as well as in married women, and the resemblance which they bear to some of the fruits of conception sometimes give rise to suspicions of pregnancy. The specimen now presented to the notice of the society had been obtained from a young woman, who had been, for a long time, suffering from painful menstruation, and for twelve months previous (since her marriage) had voided one of those membraneous growths every month, though in her maiden condition nothing of the kind had occurred. Dr. Beatty had seen a membraneous formation similar to that now produced, occurring in this patient on not less than four previous occasions. The membrane now exhibited was of recent formation and, in fact, had been passed only a few days previous. Its expulsion had been accompanied by considerable pain, and with scanty catamenial discharge. The severity of the pains and the peculiar appearance of this seemingly organized substance, gave rise to the supposition, that the female in whose case it occurred had aborted.

This growth, strongly resembled the "*decidua vera*," and in Dr. Beatty's opinion it was the result of a process similar to that which produced the true membrane. It was now generally believed that during each period of menstruation an ovum is detached, which, if not impregnated, of course loses vitality and becomes effete, and that a corresponding action is set up in the uterus for the reception of the disengaged ovum. Dr. Beatty said he believed that in those cases of dysmenorrhœa the formation of this pseudo-deciduous membrane is the result of the natural uterine effort exerted in an exaggerated degree. He said it was probable that in cases of natural and healthy menstruation there is always some membraneous formation, but it is generally so extremely thin and fragile, that it escapes notice, and passes away with the flow of the catamenia. In dysmenorrhœa, however, when the discharge is obstructed and attended with pain and symptoms of subacute local inflammation, the membraneous growth is of a more solid form, as in the present specimen, which, as regards thickness, consistence, and appearance, is similar to the *decidua vera* observable in the fourth month of pregnancy. In fact, no obstetrician, however experienced, on viewing this specimen, could take on himself to assert that it was not a result of impregnation. Dr. Beatty, being cognizant of the facts of the case, and knowing this membrane to be a consequence of dysmenorrhœa, had deemed it of sufficient importance to submit it to the notice of the society. He said it was a case exemplifying the necessity for great caution on the part of a physician in forming or enunciating an opinion on a subject so likely to involve serious considerations.

The specimen, which equalled in size the decidua vera of an ovum at the third month of pregnancy, was fresh and in excellent preservation.—*Dub. Hospital Gazette, June* 1, 1861, *p.* 172.

134.—ON DYSMENORRHŒA AND STERILITY.

By Dr. JOHN COGHLAN.

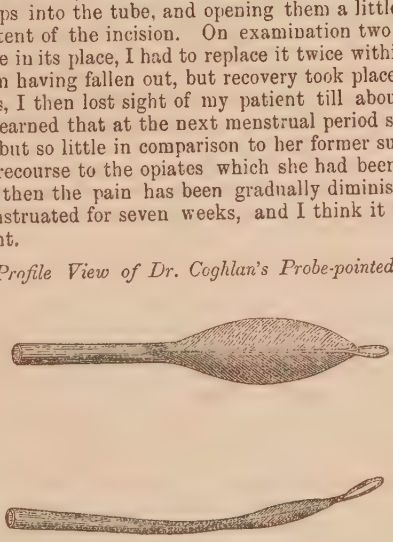
[The writer has been very successful in relieving some cases of dysmenorrhœa depending on mechanical obstruction, by incising the os and cervix uteri in a mode which is different from that usually adopted.]

Dysmenorrhœa depending on mechanical constriction of the cervix uteri, I believe to be much more frequent than some of our writers on the subject would lead us to suppose, while I consider the pure neuralgic form of the disease to be very rare. In those cases where we find obstruction at the cervix uteri, and that great nervous susceptibility exists at the same time, we are sure to have uterine and ovarian congestion, and these causes acting and reacting one on the other, produce the great amount of suffering which we find in bad cases of dysmenorrhœa. It might be thought from a patient passing an occasional menstrual period without pain, that no mechanical obstruction existed, but from careful observation I have good reason to believe that we are not justified in this conclusion. For should the subject of dysmenorrhœa, at the accession of a menstrual period, be favourably situated for the gradual secretion of the fluid, and the prevention of local congestion (confined to bed, for example) it is by no means unlikely that she may pass the period with little or no pain, though more or less of mechanical constriction of the cervix uteri, when brought into play under ordinary circumstances, may be the whole source of her sufferings. Having entertained these opinions for some years, I have from time to time treated bad cases of dysmenorrhœa by endeavouring to dilate the os and cervix uteri by means of bougies and plugs of compressed wood. These latter, from their swelling by the absorption of moisture from the parts, produced rather satisfactory results for a time, but these results were not permanent, and the same proceeding had to be repeated. I never hesitate in my practice to make a vaginal examination, or use the speculum, when I consider it necessary, but I have an utter objection to have to repeat it day after day, as in dilating the cervix uteri by bougies, for in addition to its being disagreeable to our patients, I think it has a tendency with most women to diminish their self-respect. For these reasons I have given up the practice of dilatation for a considerable time.

About eight months ago, a woman, about twenty-six years old, suffering from a very bad form of dysmenorrhœa, applied to me for relief. She stated that she had been a great sufferer from her first menstrual period up to the present, in fact, that from that one cause she had spent a life of misery. She had been four years married, had no children, and her suffering at the menstrual period had become much increased since her marriage. On examination I found the os uteri much enlarged, and presenting a flat disk nearly an inch and a-half in diameter, instead of its usual rounded form, the external orifice was

small, and the constriction was so great at the cervix that I could not pass a No. 1 flexible bougie. At a subsequent examination I carefully, but with some difficulty, passed the stylet of a catheter into the cavity of the uterus. I then passed into the cervix a small plug of pine-wood compressed by drawing it through five or six holes of a wire draw plate, and thus gained some amount of dilatation of the canal. On the following day I incised the os and cervix by means of the instrument represented below. Having perfected the incision, I took a piece of sheet lead rolled out so thin as to be very light and flexible, but sufficiently thick to retain its shape, about an inch long, and three-fourths of an inch wide, rolled it in its width into a small tube and turned the edges of the ends slightly out into a lip. I introduced this tube into the incision, and then by passing the blades of a long thin dressing forceps into the tube, and opening them a little, I dilated it to the full extent of the incision. On examination two days after, I found the tube in its place, I had to replace it twice within the following week, from having fallen out, but recovery took place without any bad symptoms, I then lost sight of my patient till about a fortnight ago, when I learned that at the next menstrual period she had considerable pain, but so little in comparison to her former sufferings, that she had not recourse to the opiates which she had been accustomed to use; since then the pain has been gradually diminishing, she had not then menstruated for seven weeks, and I think it probable that she is pregnant.

Front and Profile View of Dr. Coghlan's Probe-pointed Metratome.



This instrument consists of a central blunt or probe-point less than a quarter of an inch long, and about the thickness of a No. 1 bougie, and proceeding from this are two cutting sides about three-eighths of an inch wide. The whole instrument, including the handle, is about eight and a-half inches long, and is slightly curved for three inches from the point, to correspond with the oblique position of the uterus. The advantage of the instrument is that, guided by the probe-point, we are sure with ordinary care to have our incision right into the uterine cavity, and perfectly central, and by using instruments of dif-

ferent widths we can have our incision of the exact extent we desire. By means of the leaden tube we get rid of the disadvantage of having an absorbent, offensive, and irritating plug, and of the necessity for frequent examinations.—*Med. Times and Gazette, June* 1, 1861, *p.* 572.

135.—ON INFLAMMATION OF THE BREAST, WITH AN ANALYSIS OF SEVENTY-TWO CASES.

By Thomas William Nunn, Esq., Assistant-Surgeon to the Middlesex Hospital.

[Of the total number of 72 cases which have fallen under the author's observation, 58 occurred during lactation. Of these 57 per cent. occurred during the first two months of lactation, and 14 per cent. during the subsequent seven months, and after the ninth month 29 per cent. This appears to show that there must be a special cachexia brought about by over lactation. Both breasts are equally liable to inflammation. In speaking of the treatment the author remarked upon the following points.]

1. That the continuous poulticing so often practised or permitted was decidedly mischievous, and hindered recovery, although it was not denied that occasionally, where there was deep sympathetic pain and hyperæsthesia of the surface, large warm poultices were soothing and grateful.

2. That the recumbent position, by preventing the undue infiltration of the lower lobes of the gland, was of the first importance.

3. That in the author's hands belladonna had not given encouraging results, although he could not doubt, from the evidence of Mr. Marley and others, that in the earliest stages of congestion and as a preventive it had a beneficial influence.

4. That the moment for making the incision in the abscess, when formed, should be vigilantly watched for, as the rapidity of cure much depended upon this. That the extent of the incision should be such as to ensure efficient evacuation of the abscess, but nothing further.

5. The author strongly advocated the employment of galvanism of low intensity, such as is afforded by the ordinary single cell apparatus for yielding the interrupted current, in the treatment of sinus and painful œdema remaining after the more acute symptoms have subsided; acknowledging his indebtedness to a paper by Mr. Spencer Wells, published some years since, for the idea of so using galvanism. Whether galvanism stimulated the blood-vessels directly, or, by promoting the activity of the absorbents and the removal of the exudation products, permitted the capillary circulation, relieved of the *débris* by which it was clogged to resume its normal condition, no hypothesis was hazarded. Of the great value of galvanism as a therapeutic agent the author had seen abundant proof.

Mr. Ballard remarked that he thought the author, in assigning a cause for mammary abscess, had laid too much stress upon the health of the mother, and had not made sufficiently prominent that which seemed to be really the exciting cause of the evil—namely, a demand made upon the breast for an amount of secretion it was at the time unable to yield. The statistics quoted were in accordance with general observation, and seemed to corroborate this view, the occurrence of the greater number of cases in the earlier periods of suckling being caused by the child sucking in search of a greater quantity of milk than the gland has as yet acquired the power of secreting. Again, in the latter periods there may be a failure of secretion in consequence of the re-establishment of menstruation or of pregnancy; the demand continuing as before, the breast is exposed to the irritation of excessive sucking. Breasts of which the nipples are defective are very liable to become inflamed; their secreting power is usually very imperfect, although the determination of blood to them is the same as if they could secrete freely. Vain attempts to procure milk under these circumstances are very apt to induce inflammation and abscess. In the treatment of an abscess of the upper segment of the breast he had found the drainage-tube very useful; it enabled the pus to flow freely when otherwise none could escape.

Dr. Tyler Smith observed that the chief causes of mammary abscess soon after parturition were obstructed milk-ducts, and the irritation of the gland by fissures of the nipple. The latter caused suppuration of the glands, just in the way that irritation of the urethra or vagina caused suppuration in the inguinal glands. The best way to prevent the abscess was to cure the crack in the nipple, and he knew of nothing better than painting it with a strong solution of the nitrate of silver. Abscess depending on obstruction of the gland could often be prevented by belladonna. He had found smearing the breast with equal parts of the extract and water a better way of applying it than in the form of ointment. Bromide of potassium seemed to have a specific effect in diminishing the action of the mammary glands, both when given internally and used as a lotion. In the important class of cases first pointed out by Mr. Nunn as occurring at the end of lactation, and due apparently to fruitless sucking, the weaning of the child was always advisable. In chronic abscess, support and compression of the breast by strapping or belladonna plasters, leaving the openings of sinuses free for the discharge of matter, was of the greatest possible use.

Dr. Tanner observed as to the women who most frequently suffered from inflammation of the mammæ, that it was much more common in strumous, weakly subjects than in others, and that such labours as were attended with flooding seemed to predispose to it. Consequently he quite agreed with the author that, speaking generally, antiphlogistic remedies were contra-indicated. Then, as regards the use of belladonna in preventing the inflammation running on to suppuration

he (Dr. Tanner) had great faith in it. He thought that this drug often failed to be of use because it was not employed with sufficient freedom. The best plan was to paint the whole gland with it, and then apply a cold bread-and-water poultice, repeating the application twice or thrice in the day. As to the ill effects of over-lactation, no one who was engaged in hospital practice could fail to notice them. He remarked that he never sat in the out-patient room without seeing several cases where the women were doing themselves great mischief by undue lactation, and frequently he found remonstrance useless, as the patients knew they were acting unwisely.—*Lancet*, *June* 22, 1861, *p.* 612.

136.—ON SACCHARINE FERMENTATION IN THE MILK WITHIN THE FEMALE BREAST.

By Dr. George D. Gibb.

The discovery of vibriones in human milk, by Vogel, was announced in a paper published in 'Schmidt's Jahrbucher,' in 1853. He clearly proved that these animalcules were developed within the mammary gland, from the fact of their being seen in the milk on the instant of withdrawal. He believed them to be the result of fermentation in the milk itself, the result of congestion and increased heat in these organs, connected with general excitement of the sexual system. Vogel's theory of their formation was combated by the observation, that, as the milk containing them was either alkaline or neutral, and not acid, were there fermentation, it was argued, the evolution of lactic acid would immediately destroy the infusoria.

In the latter part of 1854, I was induced to make some researches into this important question, from the circumstance of an infant being brought to me, seven weeks old, in the most extreme state of emaciation, whose mother had the appearance of the most perfect health. The child although merely skin and bone, was healthy and plump at birth, and on very careful examination no disease could be discovered. It had never been satisfied with the large amount of milk it received, but was ravenous, and had to be spoon-fed besides. This was a first child, and the mother seemed the *beau-ideal* of an excellent and healthy nurse. The child had no diarrhœa, but the most profuse diaphoresis and diuresis had worn it to a shadow.

What was the cause of this? An examination of the milk, carefully made, at once furnished the clue. It was rich in cream, neutral, sp. gr. 1032, and showed the presence of a large quantity of sugar. So far it seemed normal. Examined under the microscope with a high power, seven hours after withdrawal, it revealed myriads of living animalcules, those indeed known as *vibrio baculus*, but which I venture to change to *vibrio lactis* as more appropriate. These were, to my mind, unmistakably the result of fermentation of the saccharine element in the milk, and might be owing to the large quantity of

sugar present; but whether occurring in or out of the breast I had yet to determine. The next day I examined the milk as drawn from the breast, and found the *vibriones* present as before, incontestably proving that the fermentation took place within the gland, as I had previously believed. There was an absence of mammary congestion and heat, but much sexual excitement, which it became necessary to control by moral and suitable medical treatment.

I did not altogether order the child to be weaned, but prescribed a diet with plenty of good cow's milk, and occasionally the mother's milk which it did not seem prudent to stop altogether. From this time the improvement began; the extreme action of the skin and kidneys ceased, the secretions became normal; and in a few weeks the child had became fat and hearty, and after a time was wholly weaned. The mother's condition also improved; but the milk always remained neutral, its specific gravity varying from 1032 to 1035, very rich in sugar, and containing the animalcules for some weeks. The richness of the milk became less as the child was gradually weaned, when it assumed a bluish tint. As quantities of it were drawn artificially, I had many opportunities of examining it with other specimens of milk; and the general result of my experiments went to prove the presence of a large amount of sugar, and that it turned sour much sooner than cow's or healthy human milk.

From 1854 up to the present time I have examined many hundreds of specimens of human milk, chemically and microscopically, and have occasionally found animalcules to be present, in that secreted from the glands of those whose general health was disordered from various causes during lactation, or where the process of lactation itself was unusually prolonged, or again where the quantity secreted was small and wholly insufficient to satisfy the wants of the infant.—*Glasgow Medical Journal, July*, 1861, p. 250.

137.—WEIGHT OF NEW-BORN INFANTS.

Dr. E. von Siebold presents a table of the weights of 3000 infants (1586 male and 1414 female), weighed immediately after birth. From this table (for which we have not space) it results that by far the greater number of the children (2215) weighed between 6 and 8 pounds. From $5\frac{3}{4}$ to 6 lbs. the numbers rose from 99 to 268; and from 8 to $8\frac{1}{4}$ lbs. they fell from 226 to 67, and never rose again at any weight to 100. From $8\frac{3}{4}$ to $9\frac{1}{4}$ lbs. they sank from 61 to 8, rising, however, at $9\frac{1}{2}$ lbs. to 21. Only 6 weighed 10 lbs., 1 $10\frac{3}{4}$ lbs.. and 2 11 lbs. The author has never but once met with a child weighing $11\frac{3}{4}$ lbs. The most frequent weight in the 3000 was 7 lbs., numbering 426. It is a remarkable fact, that until the weight of 7 lbs. the female infants exceeded the males in number, the latter thenceforward predominating. This agrees with Scanzoni's figures, who found that the

weight of the male infants in 9000 births averaged 7 lbs. ʒij. or ʒiij.; and of female infants, 6 lbs. ʒxxviij., the female infants weighing less than 7 lbs. more frequently than the males. From these statements and those of various other writers here quoted, the conclusion may be drawn that the normal weight of a mature new-born infant is not less than 6 nor more than 8 lbs., the average weight being 6½ or 7 lbs., the smaller number referring to female and the higher to male infants.

Chaussier and Quetelet have already shown that for the first few days after birth a diminution in the weight of the infant takes place, and since the beginning of 1859 the author has paid much attention to this subject, having carefully examined the weights of 49 children. In 35 of these diminution did take place, 16 of the number losing ¼ lb., 14 ½ lb., and 5 1 lb. In these last instances, however, the nutritive process was manifestly defective, and the children did not rally again, so that they cannot be included in the statement. The normal diminution of weight was observable in the 30 children from the second to the third day after birth, the weight remaining then the same to the fourth, or even the sixth day. From the fifth to the seventh day, mother and child being well, the weight which existed at birth was again attained, and then increased. In 14 of the children examined, neither decrease or increase of weight was observed until from the sixth to the eighth day, when increase commenced. The mean of Quetelet's observations is thus stated:—The child weighed at birth 3126 kilogrammes; on the second day, 3057; on the third, 3017; on the fourth, 3035; on the fifth, 3039; on the sixth, 3035; and on the seventh day, 3060 kilogrammes. This diminution in weight in most of the children, and the arrest of its increase in others, is evidently dependent upon the change in its mode of nutrition on arriving in the world. The character of the early milk is very different to that which it afterwards assumes, containing more colostrum corpuscles than milk globules, and possessing rather a purgative than nutritive action. Not only is the child's weight found to diminish, but its entire body gives signs of the defective nutrition, the redness of its surface being exchanged for one of a yellow, or at least a white colour, while there is also observable a certain relaxation of its limbs,—signs which all disappear again in a few days. The author is sensible that his numbers are too small to enable him to arrive at any decisive conclusions, and he appeals to other obstetricians for their co-operation in the investigation. The length of new-born infants exhibits much less difference than does their weight. Of the 3000 children measured, the length varied from 15 to 21 inches; but out of the number no less than 1674 measured 18 inches, and 695 17 inches; so that the mean length was between 17 and 19 inches. As in the case of the weights, so in the lengths, the females exhibited the lesser numbers. Thus, of 695 children which measured 17 inches, 380 were females, and 315 males; while of the 1674 which measured 18 inches, 867 were males, and 807 females. Of the 305

children 19 inches long, 198 were males, and 107 females; and of the 49 which measured 20 inches, there were 37 males, and only 12 females. Elsasser and Quetelet observed similar results.—*Medical Circular, April* 10, 1861, *p.* 259.

138.—RESEARCHES ON ASPHYXIA, WITH OBSERVATIONS ON THE EFFECTS OF THE HOT BATH.

By Dr. A. T. H. Waters, Lecturer on Anatomy and Physiology in the Royal Infirmary School of Medicine; Physician to the Northern Hospital, Liverpool.

Amongst the numerous researches which had been made with reference to asphyxia, the author was not aware that any had been directed to one of the subjects he had investigated. Physiologists are agreed as to the order in which the arrest of the vital action takes place in asphyxia, but not as to the duration of the heart's action, nor yet as to the best mode of treatment in suspended animation. The different societies, whose aim is to save life, issue rules of the most opposite character for the restoration of those apparently dead.

Two important points remain to be decided: first, the period after asphyxia has commenced during which treatment is likely to be successful in restoring animation; and, secondly, the value of the hot bath as a remedial agent.

Experiments had been instituted by the author with reference to the following questions:—

1. How long does the heart continue to beat in asphyxia?

2. What are the effects of the hot bath on an asphyxiated animal—firstly, after all respiratory movements have ceased, and are not re-excited; secondly, when respiration has been re-excited, and is being feebly carried on?

It is difficult to decide with any degree of certainty with regard to the first question. Certain circumstances tend to throw a doubt on the generally received opinion, that "in asphyxia the movements of the heart cease in a few minutes after the cessation of the functions of animal life." The second question is more readily answerable, but the author was not aware that any experiments bearing directly on it had been performed previous to his own.

The subjects of experiments were dogs, cats, and rabbits. They were drowned in water varying in temperature from 40° to 50° Fahr., and in one instance 36°. On being removed from the water, after every external symptom of life had disappeared, they were opened by the removal of the anterior part of the chest, so that the movements of the heart could be observed.

Some of the experiments were parallel—i.e., two animals of the same age and size were chosen, and after being drowned in the same way, were opened at the same time; the difference being that one ani-

mal was, previous to being opened, put into the hot bath at 100°, and the other was left exposed to the atmosphere. The number of animals experimented on as just mentioned was 28; of these, two being set aside which were submerged for an hour, in 18 the heart was found beating when first observed; in 8 its action had ceased.

The animals were opened at periods from the commencement of asphyxia varying from the fifth up to the thirteenth, and in one instance the twenty-first minute. The average period during which the ventricles continued to contract was nineteen minutes; the longest period was in a rabbit—forty-five minutes.

With regard to the first portion of the second question, the morbid appearances of the animals put into the hot bath were compared with those of the animals not so treated. In the animals put into the bath, the lungs were much more congested, more full of blood; they were firmer in substance and specifically heavier than those of the animals not so treated. Both sides of the heart were loaded with blood. In some instances the blood was coagulated in the vessels of the lungs, the systemic veins, and the cavities of the heart. The blood was generally less fluid than in the animals not put into the bath, and coagulated more rapidly when removed from the vessels. In no instance did the bath produce a respiratory effort or any movement whatever on the part of the animal; it seemed, further, to shorten the duration of the heart's action.

With regard to the second portion of the second question, experiments of the following character were performed:—

Animals were drowned in water from 45° to 50° Fahr.; they were kept under water, some for one minute, some for one minute and a quarter, and one for two minutes. When removed from the water, they were placed on the table exposed to the air. They soon began to breathe feebly. In order to compare the effects of the hot bath with those where all treatment was omitted, some of the animals were left to themselves, others were put into the bath as soon as respiration had been re-excited.

Thirteen experiments were performed—twelve on rabbits, one on a cat. Of the thirteen, seven were put into the hot bath; of these, six died, at periods varying from two to twenty hours after submersion. Six animals were left to themselves; of these, four recovered, and two died, both between the eighth and twentieth hour after submersion.

The animals which had died after being put into the hot bath presented the following morbid appearances. The lungs were dark-coloured, full of blood, firm, almost liver-like in appearance. In some instances, portions sank in water; the air-tubes were empty; there was blood in all the cavities of the heart, in one instance coagulated. In the animals which died after submersion, but which were not put into the hot bath, the lungs were somewhat firm and congested, but to a less extent than in the others.

The experiments appear to the author to be sufficient to establish

the principle of the injurious influence of the hot bath, both when asphyxia is complete and when recovery is commencing. In the former case, the bath momentarily increases the circulation; but respiration being in abeyance, the lungs become loaded with blood, and the left side of the heart distended. In the latter case, respiration being imperfect, engorgement of the lungs takes place, and subsequent arrest of the heart's action.

The following conclusions are drawn by the author from his experiments:—

1. That in asphyxia by submersion, the ventricles of the heart do not, as a rule, cease to contract "in a few minutes after the cessation of the functions of animal life," but that in many instances their action continues for a very considerable period, and that this serves to explain how recovery has taken place after lengthened submersion.

2. That in cases of asphyxia where respiration has altogether stopped, the effects of the hot bath are: to produce an accumulation of blood in the lungs and in the left side of the heart, together with a tendency to coagulate on the part of the blood; that it does not tend to prolong the action of the heart, but rather to paralyse its movements, and diminish the duration of its contractions; that it does not excite respiratory efforts, and prevents artificial respiration being properly carried out.

3. That in cases of asphyxia where respiration has been re-excited and is being feebly carried on, the hot bath, although in some instances it seems to have no immediate bad result, yet has a tendency to produce a fatal issue some hours after its use, by causing extreme congestion of the lungs, together with consolidation and collapse of of the pulmonary tissue.

The following practical inferences are drawn from the above conclusions:—

1st. That efforts to restore animation should be made in all cases where asphyxia has not been of very prolonged duration.

2nd. That the prolonged use of the hot bath in asphyxia is not only inefficacious, but dangerous; and that its temporary use appears to be attended by no direct benefit. So far as any means similar to that of the hot bath are likely to produce respiratory movements, the alternate dashing of hot and cold water on the body is probably the most efficacious.

3rd. That it appears safer practice to omit all artificial treatment, when respiration is going on feebly, than to make use of the hot bath.

4th. That in the treatment of asphyxia all efforts should be primarily directed to restoring, or continuing, the respiratory movements; and all measures tending to load the lungs or embarrass the respiration should be avoided.

The author believes that the best method of performing artificial respiration we are yet acquainted with is that recommended by Dr. Marshall Hall.

A letter was read at the Royal Medical and Chirurgical Society, from Sir Benjamin Brodie relative to the paper 'On Asphyxia and the use of the Hot Bath,' by Dr. A. T. H. Waters. The letter referred to the question of the period of the continuance of the heart's action in cases of asphyxia. Sir B. Brodie had never, in his experiments on the subject, known the rhythmical contractions of the heart to continue for more than four minutes and a half after complete submersion; and believed that if they had once ceased in asphyxia they could not be restored. Cases of recovery after a longer submersion he attributed to the action of the heart having previously ceased in a state of syncope.—*Lancet, May* 25, *and June* 8, 1861, *pp.* 513, 563.

139.—THE ATROPHY OF CHILDREN FROM DIRT EATING.

By Dr. William Moore, Physician to the Institution for Diseases of Children, Physician Extraordinary to Sir P. Dun's and Mercer's Hospitals, &c., Dublin.

Travellers have long since informed us of the earth-eating habits of the natives of many tropical countries, more particularly those of the islands in the Indian Ocean, where pregnant women made a practice of baking, roasting, and eating a reddish clay, impregnated with iron. Humboldt has mentioned earth-eating as common to various tribes in South America; he especially refers to the Ottomaques, who, during the floods in the Orinoco, which prevent their catching fish, eat a peculiar clay; these savages, by no means an unhealthy race, considered this clay nourishing, and Humboldt doubted whether or not portions of it might not be assimilated. Again, fugitive negroes in the West Indies have been known to subsist on earth for a considerable time, and a case is mentioned by Mr. Mason, of a fugitive negro boy, who, when conveyed to the property to which he belonged, was found to have swallowed a long rag of blue baize, the end of which projected from his mouth. Psychological reports have detailed extraordinary instances of lunatics indulging in the most indigestible and disgusting commodities with comparative impunity. The following cases which have come under my observation within the last few years, tend to show what a destructive habit or rather disease pica or dirt-eating in children becomes. The first case I shall adduce was that of Ann F. V., aged two and a-half years, who was brought to the Hospital for Children, Pitt-street, in November, 1859. The child's lips were anemic and bloodless, her skin of a yellowish hue, her expression downcast and forbidding; on examination I found palpitation, a tumid, irregular abdomen; bowels at times lax, at others obstinately constipated; discharges generally of a pale, plastic, offensive character. On inquiry, I learned that this child, when left to herself, eat gravel, lime, cinders, in short, every refuse she could lay hold of. The treatment

employed in this case was repeated enemata of soap and water, which brought away gravel, stones, and indigestibles of all sorts, at the same time small doses of grey and rhubarb powder were given night and morning, and a tepid bath at bed time. Under this treatment with careful watching to prevent her satisfying her vicious craving, this child was gradually restored to health, which improvement was further confirmed by the exhibition of syrup of iodide of iron and tepid sea-water baths.

Case 2.—Was that of William T., aged two years, a chlorotic boy, lips and gums blanched and anemic, with deep unhealthy fissures at each angle of the mouth; skin generally dry; his look dull and drowsy, and his temper very irritable. On examining the abdomen, I found dullness over the right hypochondriac and even down to the iliac region; the belly was tumid, and there was evidence of slight effusion. The boy's extremities were œdematous. Diarrhœa had been persistent for weeks despite of all treatment. The mother of this child attributes his malady to eating earth, lime off the wall, and gravel, the passing quantities of which, by stool, first made her aware of the habit. The treatment adopted in this case was three grains of the powder of chalk with opium, twice daily, with tepid mustard baths at bed time, beef-tea and wine liberally; but all failed to control the diarrhœa; the case went on from bad to worse, and eventually proved fatal.

Case 3.—In August, 1859, I was consulted about Michael B., aged five years, a fine stout boy, with a most remarkable expression; he is unwilling to raise his head or look up, and when obliged to do so, there is a heavy, vicious look about him, so unnatural to a child of his age; there is not a general sallow hue of the skin, but a circle round each eye, engaging the upper part of both cheeks, partakes of that character. This child's mother has used every means to prevent him eating refuse in general, but coal and cinders in particular, and even when his hands have been sewn up in linen or cotton bags, he has been found on all fours trying to get his head under the grate; he was always a very violent tempered child, and latterly has become even more so. The abdomen in this case was hard, but not irregular or tumid; occasionally he suffers from diarrhœa, when the dejections are of a putty-like character and in enormous quantity; when I first saw him, his bowels had been constipated for days. This lad took two grain doses of grey powder, with rhubarb and scammony, at bed-time, followed by infusion of senna the next morning, till the bowels were well cleared of all their impactions; he got a tepid bath nightly for weeks, and a servant was set apart to prevent him indulging his vicious craving. Under this treatment his secretions were restored to a healthy condition; he lost the yellowish circles round the eyes, and, pari passu, the downcast expression. He is now a fine rosy-cheeked boy.

Case 4.—Mary M., aged two years, a cachectic child, of a pale straw colour, covered with lichen, was brought to the Pitt-street Insti-

tution for Children in March last. This girl's mother has failed to prevent her swallowing coal, pieces of glass, and prawn shells; except for some such unnatural edibles the child has no desire. The general atrophy, coupled with continuous diarrhœa of a slimy, bloody character, rendered the prognosis of this case far from hopeful; however, under the continued use of rice milk, with small doses of the compound powder of chalk with opium, and tepid bran baths at bed-time, the diarrhœa was controlled, and the lichen disappeared to a considerable extent.

Case 5.—Ann B., aged two years and three months, was brought to me in September last. From the downcast appearance, or rather loss of expression, the fixed and sluggish state of the lids, coupled with the yellowish anemia, led me at once to suspect the origin of the mischief. The abdomen was hard and enlarged, the child was suffering from diarrhœa, the dejections, as her mother described them, being like "bad eggs." This child's craving was principally for earth and gravel. Under the use of small doses of grey rhubarb and aromatic powder, with tepid mustard baths at bed time, and close watching as regards her diet, this child has regained a healthy look, and her expression is considerably restored; she has latterly been taking syrup of the iodide of iron.

Case 6.—Patrick C., a little over two years of age, was brought to Pitt-street within the last month; a pale, sallow child, with loss of expression; he has been in the habit of eating small pieces of bricks, lime, and earth for the last six months; his breathing is hurried; he suffers from palpitation and incessant diarrhœa, from twelve to sixteen times during the day and night, the fæces being slimy, bloody, and jelly-like, and extremely fetid; he is ascitic; and his extremities emaciated. Under the combination of grey and compound powder of chalk with opium, with mustard sitz baths at bed time, rice-milk, and strong beef-tea as diet, he seems to be improving, but as yet the amendment is not decided. The most remarkable features diagnostic of dirt-eating in children is the fixed, vacant, or downcast look, any expression remaining being forbidding, occasionally vicious. In most of these cases there is an unwillingness on the part of the child to raise the eyelids, and from this cause the levatores palpebrarum become atonic or semi-paralyzed from disuse, the lids drooping over the eyes; coupled with these signs, you have anemic lips and gums, a tinge of the cutaneous tissues, usually varying from a straw to a deep yellow; in addition, the arrest of secretions generally; with palpitation; tumid, irregular, occasionally ascitic abdomen; and intractable diarrhœa or the converse, leave little doubt as to the "prima origo mali." The pathology of these cases usually present hepatic enlargement, the gall-bladder filled with dark bile; in other respects the phenomena are similar to those of fatal cases of strumous peritoneal and mesenteric disease, occasionally pleuritic and meningeal effusion hastening the fatal issue.

As regards the treatment of these cases, our first efforts should be directed to dislodge the offending mass, and for this purpose I think enemata are peculiarly suitable; calomel or grey powder being simultaneously exhibited. Dr. Corrigan, who has recently contributed some most instructive cases on pica, recommends calomel in repeated doses, followed by carter oil and turpentine. Once the offending matters are removed, our aim should be directed to improve the character of the secretions generally, and for this purpose the alterative powders I have above mentioned, with the continued use of tepid baths, will be found most efficacious. To control the diarrhœa, I prefer the steady exhibition of the pulv. cret. opiatus; and in the convalescent stage the syrupus ferri iodidi, vinum ferri, or other chalybeate as indicated. Of course, the most careful watching, as regards diet, and to prevent the vicious propensity being indulged in, is indispensable.—*Dublin Hospital Gazette, Feb.* 1, 1861, *p.* 33.

140.—ON CEREBRAL DISEASE UNATTENDED BY CEREBRAL SYMPTOMS.

By Dr. Charles West, Physician to the Hospital for Sick Children.

[In phthisical or tubercular subjects, there is much risk of overlooking the advances of cerebral mischief. It is by no means always that the pain points to the seat of mischief.]

Solicitude has perhaps existed for some time with reference to the child's health; slight cough is present, and with it there is some loss of flesh. Presently the cough increases a little, and the parents attribute this to the child somehow having caught fresh cold. There is no marked head-affection, the child indeed is heavy and dull, but makes no special complaint of headache, or says at the most that his head hurts him when he coughs. The bowels are often somewhat constipated, but probably a disposition to constipation is habitual, while if vomiting occurs once or twice it is referred to the expectorants prescribed for the cough, and ceases either spontaneously, or as the result of some change in treatment. But still the patient is no better; the pulse continues rapid and feeble, the heaviness and dulness deepen into a constant drowsiness, the drowsiness into a stupor, and the stupor issues in coma which may or may not be preceded by convulsion; the riddle of diagnosis being solved by the evident head-symptoms which accompany the last few days or hours of the patient's life.

A girl, seven years and nine months old, who inherited a phthisical tendency from her maternal ancestors, was attacked by hooping-cough four months before she came under my notice. In the course of a week measles supervened which were complicated with severe diarrhœa; while the hooping-cough lasted, though with no great severity, for four months. A month after its cessation the child, who still had some cough, was admitted into the Children's Hospital. She was

very weak, much emaciated, had various dyspeptic symptoms, and presented all the characters of advanced phthisis, while there were evidences of tubercular deposit in the left lung. Her pulse was a hundred in frequency and very feeble; her tongue was much coated, her lips were dry, and she resembled not a little a patient with typhoid fever: between which disease and acute tuberculosis, I may just remind you in passing, the diagnosis is often by no means easy. She slept well the night after her admission, and made no complaint of headache, but in the morning she was sick once, a symptom which had occurred before, but which at no time was frequent or troublesome. On the following day she made slight complaint of headache, but her face was tranquil, and though not sleeping much by day she was always quiet. Her pupils on this day were observed to be rather more dilated and rather more sluggish than they ought to have been. Sickness had not returned, nor did it; but the complete distaste for all food and drink was very remarkable. On the fourth day all headache had disappeared, but drowsiness seemed creeping on; it deepened into coma on the sixth day; and, undisturbed, save by very slight twitching of the muscles of the face, on the eighth day the child died.

The emaciation was extreme, and there was abundant tubercular deposit in almost all the organs of the body, especially in the bronchial glands and spleen. There were traces of old mischief at the base of the brain in the form of loss of transparency of the membranes, and some deposit of tubercular granulations along the fissure of Sylvius. The ventricles did not contain above six drachms of fluid, and there was no softening of the central parts of the brain, while the congestion of the arachnoid was not considerable. In short, the post-mortem appearances harmonized with the symptoms during life in illustrating the fact that whenever the tuberculous cachexia is much developed, a very small amount of cerebral disease is quite sufficient to destroy the patient; and the slight symptoms during life will be found to be the expression of a very small amount of local mischief.

The younger the child, too, the smaller will usually be the amount, both of general and local disease that will suffice for its destruction. There are also certain epochs of infantile life at which unusual demands are made on constitutional vigour, and at which, therefore, more than ordinary watchfulness is called for, and symptoms have a more than ordinarily grave significance; such are the epochs of weaning, and of dentition; and one of the reasons for somewhat protracted suckling of a weakly infant is to prevent the coincidence of the irritation of teething with the constitutional disorder that too often accompanies the first attempt at the substitution of artificial food for the mother's or nurse's milk.

It is at this time that slight attacks of illness are apt to pass unnoticed, or to be referred for an easy solution of their meaning either to the change of diet, or to the irritation of teething. The

child becomes fretful, sometimes feverish, generally restless at night, but still there are intervals of almost its wonted cheerfulness. There is no marked intolerance of light,—a symptom, by-the-bye, the existence of which it is not easy to ascertain in the infant; constipation is seldom obstinate, often does not exist at all, while loss of appetite and refusal of its usual food are often even more marked than actual vomiting. For days, sometimes even for weeks, these symptoms continue, affording nothing definite enough for treatment scarcely definite enough to call for grave solicitude. At length with no apparent cause, there sets in an increase of the fever, attended by considerable heat of head and frequent outcries. These last for a variable time, for two or three days, or less, or sometimes even only for a single night, and the next day brings quiet; the child is drowsy after its suffering of the day before, so it seems at least, and the drowsiness deepens, and then coma steals on so almost imperceptibly that the anxious mother clings to the confident expectation of her little one's recovery for days after the doctor has seen the certain issue of the disease in death. Now and then, too, the acute stage of excitement and pain and fever is altogether wanting, and the transition from mere discomfort, first to apparent ease, and then to drowsiness, then to sleep, and from sleep to stupor, from stupor to death is so gentle, that not the friends only, but the doctor, too, are apt to be taken by surprise.

And how is such surprise to be avoided? Partly by bearing in mind that the ill-marked febrile disorders—the fretfulness, the unrest, all the assemblage of indefinite symptoms which I have mentioned—is commonly indicative of the advance of tubercular deposit, or, at any rate, accompanies that state of health in which deposit of tubercle is most apt to take place. Next, by remembering that in the infant at the period of dentition, there is a special source of irritation of the nervous system which tends to render the brain most sensitive, and its functions most likely to become disordered. Lastly, that this being the case, any signs of constitutional disturbance not clearly traceable to some other cause are most likely due to disease of the brain, and that consequently, on every occasion, you are bound to disprove the existence of cerebral disease before you are at liberty to seek further for an explanation of the symptoms.

But it is not in comparatively acute cases only that you are liable to fall into error, owing to the absence of those pathognomonic symptoms, which usually you expect to meet with. The course of chronic disease may also be obscured in a similar manner, and this even when the appearances disclosed after death are such as to make it seem almost impossible that disease so grave should not have announced its presence most emphatically by symptoms of corresponding intensity.

Not many months ago a boy, ten years old, was admitted into the hospital who had been ailing for six weeks with occasional vomiting and a relaxed state of the bowels, which, for some days, had amounted

to rather severe diarrhœa. He had lost his appetite and grown thinner, and had rested ill at night, but pain referred to the back of the head, nearly constant but never intense, was the only symptom which appeared indicative of cerebral disease. After a continuance of these symptoms for six weeks, he was admitted into the hospital as much on account of his failing health as of any solicitude excited by the headache. He was perfectly intelligent, and distinctly referred to the occiput as the seat of pain; but there was no tenderness on pressure there, or about the upper part of the cervical spine. A sluggish condition of the pupils, which were more dilated than natural, and slight divergence of the eyes existed, but neither of them to such an extent as at all to have forced itself upon observation.

He survived his admission sixteen days. His appetite improved; for a few days his headache considerably lessened, though it did not altogether cease, but then again returned, and was referred alternately to the occiput and forehead. His improvement occurred while taking small doses of the iodide of potassium, but I do not imagine that any considerable importance is to be attributed to it: and a blister was applied to the nape of the neck, on account of some increased headache two days before he died. Death took place unexpectedly, and so gently, that the attitude of quiet slumber in which the boy was seen more than once during the night was undisturbed.

A large mass of yellow tubercle, a little irregular, but about two inches in diameter, was imbedded in the left hemisphere of the cerebellum, pressing on and distinctly flattening the medulla oblongata. There was no change in the brain-substance in the neighbourhood of the tubercle, or elsewhere, but the ventricles were much enlarged, their lining membrane was tough, and they contained eight ounces of perfectly transparent fluid.

It is worthy of note, that with the exception of slight old tubercular deposit at the apex of the right lung, the body presented no other trace of it. This is of importance, inasmuch as it shows that extensive disease in the brain may exist, independent of those general evidences of tuberculosis which often serve as a clue to the understanding of symptoms otherwise obscure.

In this case one symptom, and one only, was present to fix the attention on the probable existence of disease of the brain, and that was pain in the head. It was not intense pain, it was not sudden in its onset; brief in its stay, complete in its disappearance, relieved by attention to the state of the bowels, or even ceasing of its own accord, as did those neuralgic headaches which I spoke about at the last lecture, but it was constant, varying indeed, now better, now worse, but never entirely absent.

Be the symptom of cerebral disease, then, what it may, the mere fact of its persistence attaches to it an importance which it is almost impossible to overstate, and which far exceeds what would belong to it, even though ten-fold more characteristic when present, if that pre-

sence were occasional and interrupted by intervals, during which it ceased completely. Of no symptom is this more emphatically true than of pain as a sign of cerebral disease.

In the great majority of instances the existence of any considerable deposit of tubercle in the brain produces not merely pain, but also impairment of power over some of the voluntary muscles, showing itself in paralysis, or in convulsive movements, or in both. It is unusual for the latter to precede the pain, and still more unusual for them to continue for any considerable time without becoming associated with pain and with other unmistakeable evidences of cerebral disorder.

Such exceptional cases do, however, now and then occur. A little girl, one of five children of healthy parents, in whose family no phthisical predisposition existed, had had uninterruptedly good health, not disturbed even by the process of teething. While at breakfast, when two years and a-half old, and apparently in perfect health, her mother noticed a tremulous movement of the right hand upon the wrist. In fourteen days the movement extended to the right arm also, and in another month to the right leg. At two years and nine months she came under my notice. At that time the leg and arm trembled in an almost equal degree, and the foot was drawn inwards. The child could stand, but could not walk; she could raise her hand to her head, could hold anything very light, but nothing of weight, in the hand. The tremblings ceased when in profound sleep, but the slightest irritation of the surface reproduced them without waking her. For about five weeks before I saw her, her head had been constantly drawn towards the left shoulder, though it became quite straight when she was asleep; and she had some power to elevate it, though she was not able to keep it raised. When these symptoms first came on she seemed somewhat out of health, lost flesh, slept badly, and her appetite failed; but for a month previous to her admission she had seemed well in all respects with the exception of the involuntary twitching.

The child was perfectly merry all day long, and some degree of constipation was the only symptom of disorder of her health. Aperients were given to her, but no other medicine. In five days there was marked diminution of the wry neck, and in nine the twitching of the arm and leg was also lessened. By degrees she acquired the power of standing firmly with very slight support, though, as often happens with children suffering from chorea, any excitement, or the consciousness of being watched, sufficed to bring back all the tremulousness almost in as great a degree as when admitted into the hospital. Simultaneously with this improvement, however, it was observed that the mouth was drawn somewhat to the left side; but this symptom gradually abated, and finally disappeared in the course of the next month. She became able to walk firmly, and to grasp objects well in her hand; and she left the hospital fourteen weeks after her admis-

sion with no other ailment than slight tremulousness of the hand and arm, and somewhat impaired power over the right leg. For five weeks only this improvement lasted, but at the end of that time she began to walk less well, gradually grew heavy and drowsy, and sank, at the end of ten days. into a semi-comatose condition, in which both pupils were dilated, but the right especially so. Convulsions then came on, accompanied by rigidity of the left limbs, while the right were in violent movement. Slighter convulsions followed, in which both sides of the body were equally disturbed ; but from the fourth to the sixth day there was a partial return of consciousness, which once more gradually disappeared. The right arm became permanently rigid, while automatic movements took place of the left arm, and of both legs. On the seventh day deglutition became difficult ; on the ninth, impossible ; and on the tenth day from the first occurrence of coma death took place.

The cerebral convolutions were flattened, the arachnoid on the convexity of the brain was granular, and much fluid escaped on removing the brain. There was abundant effusion of blood beneath the arachnoid at the base of the brain, covering both surfaces of the medulla oblongata, reaching as far forward as the optic commissure, and downwards, though slightly, on the posterior surface of the cord, as far as the fourth cervical vertebra. There was a softened tubercle of the size of a pea at the posterior part of the right hemisphere of the cerebellum, and there were five masses of tubercle in the substance of the left optic thalamus, but no softening nor other appreciable change in the cerebral substance about the deposit. Tubercle, but to no considerable extent, was present in the lungs and bronchial glands.

In this instance, irregular, almost choreic, movements were for some months the only evidence of the grave cerebral disease which, advancing all the time, at length destroyed the patient. But even they might serve to excite suspicion. They were all the time limited to one side of the body, while chorea seldom continues so restricted, though one side only may be at first attacked. The drawing of the head towards the left side, and afterwards the paralysis, even though temporary, of the right portio dura, pointed to some abiding irritation of the brain, and served to raise the suspicion that the symptoms were of a graver import than at first seemed likely, if one looked merely to the apparently good health and the perfect happiness of the little child.—*Med. Times and Gazette, April* 20, 1861, *p.* 411.

MISCELLANEOUS SUBJECTS.

141.—ON THE USE OF ALCOHOLIC STIMULANTS IN HOSPITAL PRACTICE.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary, Edinburgh.

[Dr. Gairdner seems to have taken an accurate account of the quantity of alcohol which in each case he prescribed. Before adding up the whole, he suspected that at any rate he had not been increasing the quantity, but to his surprise he found that there had been a nearly uniform increase, especially in the female wards. We think that Dr. Gairdner's view of the use of alcohol is the true one, and opposed to what is commonly understood of Dr. Todd's views. Wines and alcohol "are *stimulants* and *tonics*, *i.e.*, *medicines*, and not *food*, properly so called;" and to administer them in the early periods of acute disease requires the greatest caution. Dr. Gairdner says]

My own habitual practice has been to give stimulants, if at all, only in very moderate quantities along with the food, and, in general, as an aid to the digestion of food,—the only exceptions being in the case of persons largely and habitually dependent upon stimulants from old and formed habits, and in a comparatively small number of acute cases for a very few days, sometimes only for a few hours, to help the system over a dangerous crisis, or to co-operate with other needful remedies, such as antimonials in pulmonary inflammation. I feel quite assured that there have been no such facts in my experience of alcoholic stimulants as are mentioned in Dr Todd's last volume—*e.g.*, brandy, at the rate of 6 drachms every hour, given to a girl of 17 years of age in rheumatic inflammation (case of Jane Cook); or, in another case (Sarah Butcher, age and habits not stated), a pint a day of brandy for a month together in pyæmic inflammation. Such facts are, I cannot help thinking, the indications of a great excess, if not of an entirely wrong direction, in the use of these powerful remedies; which, to be powerful for good and not for evil, must be maintained strictly within the limits of their medicinal action, and given, not *as being food* in themselves, but rather as *adjuvants to food—i.e.*, as aids to the gastric digestion, and stimulants of the nervous system and circulation. I quite agree, however, with Dr. Todd in thinking that when stimulants really act beneficially in acute disease, they diminish the frequency of the pulse and restrain the tendency to delirium, while improving the appetite and producing an amendment in all the general symptoms. On the other hand, I cannot but demur to his inference that delirium

and other bad symptoms, even if increased under small doses, are to be kept down by giving much larger quantities. To regard flushing of the face and increased feverishness, for example, as not a contra-indication, but a reason for increased administration (Lecture viii., p. 269), is opposed entirely to the practice I have followed; for, under such circumstances, or even when stimulants have not been obviously followed by reasonably good effects after a cautious trial of small quantities, I have nearly always abandoned them at once as being unsuitable remedies, at least for the time; and, accordingly, it has never occurred to me to have to "sluice the head well with cold water," or to use any of the other means recommended in Dr. Todd's fourteenth lecture, in order to distinguish "the coma of alcohol" from "the coma of disease" in cases of accidental over-stimulation.

Let me add, that another opinion prominently put forward in Dr. Todd's book appears to me to demand qualification, viz., that "it is far more dangerous to life to diminish or withdraw alcohol than to give too much." So far from having had constantly before me the fear of sacrificing life by diminishing or withdrawing a habitual allowance of stimulants, I have made it part of my regular practice to do so in most cases of persons accustomed to the use of ardent spirits in excess, and especially in many cases of delirium tremens, or of other acute diseases modified by alcoholic excesses; and though not committed to the treatment of any disease entirely without stimulants, I can entirely corroborate the remarkable statements made by Dr. Peddie in his very important memoir on delirium tremens, viz., that the suppression of the habitual allowance is not, *per se*, dangerous in most cases; but on the contrary, extremely conducive to the cure. In regard to other diseases, I believe that much more mischief is done by the routine administration of stimulants than could possibly result even from their entire suppression in hospital practice. In the case of young persons affected with fever, indeed, it has been almost made matter of demonstration by statistical data, that the stimulating practice, pursued on Dr. Todd's plan, was the opposite of useful in the saving of life. Nor can it be doubted that in pneumonia, as well as in fever, Dr. Todd's practice, as recorded by himself, was less successful than that of many practitioners who are less liberal in the administration of alcoholic stimulants. It is quite true that comparisons cannot always, or indeed often, be drawn with accuracy from limited numbers of cases in different spheres of observation; nevertheless, I feel well assured that the recorded mortality from the cases of pneumonia or fever occurring under Dr. Todd's care in King's College Hospital, would be regarded as excessive in the Edinburgh Royal Infirmary. On this subject it is sufficient for the present purpose to refer to the "Five Years' Hospital Experience of Pneumonia," published in the 'Edinburgh Medical Journal' for March 1860; and to the 'Clinical Notes on Fever,' in the same Journal for July 1859, although neither of these papers were compiled with a

view to the present inquiry: nor would it be fair to Dr. Todd to assume that his cases were parallel to those referred to in these papers.

I may, in conclusion, be permitted to refer to the 'Edinburgh Medical Journal' for February 1858. The review of the arguments at p. 735, *et seq.*, in regard to the medical uses of alcohol, is so expressive of the views I have all along held on this subject, that I am well satisfied now simply to lay claim to the authorship of this anonymous paper as evidence that the opinions now put forward had been carefully considered long before this paper was thought of. The recent progress in the physiology of the subject, and especially the researches of Lallemand, Perrin, and Duroy in France, and of Dr. Smith of London, appear to me to be in general confirmatory of the ideas expressed in that review; although, no doubt, some of the questions in controversy may still for a long time remain open to discussion on points of detail.

The object of this paper will be served if it shall be the means of procuring more accurate records than hitherto of the actual expenditure of alcoholic stimulants in hospital practice. Considering the vast moral issues involved in this question, and considering also the important economic interest which the governors of our public charities have in keeping within reasonable bounds the administration of stimulants, it is surely not too much to suggest that in every hospital in this country monthly returns should be made, exhibiting, as in the Edinburgh Royal Infirmary, the aggregate expenditure in each ward, and also a calculated average of the amount supplied to each individual patient. By such averages, physicians would be insensibly

Average Daily Consumption of Alcoholic Stimulants per Patient during Five successive Years, in the Royal Infirmary, Wards, 4, 15, and 16.

	1856.	1857.	1858.	1859.	1860.
General Ward, Males—					
Wines (ounces)	0·158	0·465	0·710	0·928	0·739
Spirits (ounces)	0·056	0·312	0·287	0·184	0·454
Malt Liquors (pints) ...	0·039	0·040	0·025	0·053	0·058
General Ward, Females—					
Wines (ounces)	0·446	0·534	0·799	1·498	1·200
Spirits (ounces)	0·295	0·312	0·223	0·164	0·510
Malt liquors (pints) ...	0·064	0·069	0·048	0·061	0·048
Fever Ward, Females—					
Wines (ounces)	0·715	1·256	1·734	1·725	1·140
Spirits (ounces)	0·069	0·083	0·346	0·052	0·135
Malt liquors (pints) ...	0·023	0·029	0·135	0·069	0·027

guided to the truth ; and the results of various practice would, when carefully compared, supply data hitherto wanting for the settlement of a great many scientific questions connected with alcoholic stimulants.—*Edinburgh Med. Journal, May* 1861, *p.* 961.

142.—ON THE ACTION AND USES OF ALCOHOL.

By Dr. Daniel Hooper, B.A.

How do alcoholic liquors act on the human body—as foods or as poisons?—and is their regular and moderate use beneficial or injurious? These are questions to which our most eminent physicians and physiologists give contradictory answers, and the intelligent and reading classes appeal to them in justification of their habits of indulgence or abstinence; they take or avoid alcoholic liquors systematically, and on principle, and the practice in either case is based upon a scientific truth or a scientific error. It is therefore of the highest importance, both for the sake of the public health and of science, that those to whom we naturally look for guidance in these matters should come to some definite and determinate conclusions on the alcohol question. What can be more deplorable than the fact that the medical profession is divided into two distinct schools on this subject—the one represented by Dr. Carpenter, and the other by Dr. Todd, Dr. Chambers, &c.? By one school alcoholic drinks are regarded as *food*, by the other as *poison*; by one as a regular, daily, and useful, if not necessary, part of man's sustenance ; by the other as powerful medicinal or poisonous agents, only useful, like those of the apothecary, under certain rare and peculiar circumstances. One school would place them in the same category as salt, sugar, butter, and tea, as articles of ordinary domestic consumption ; the other would range them on shelves, in company with laudanum, sal volatile, and chloroform. These facts prove that a deeply-rooted fundamental creed as to the action of alcohol upon the human tissues lies at the bottom of, and gives its peculiar tinge or complexion to, each of these schools; and until this action of alcohol upon the tissues and vital functions is finally settled and agreed upon, it is impossible that the teaching of men of science and the practice of the public can be, as in the case of meat and bread, unanimous and satisfactory.

It is extremely difficult to determine, with accuracy and precision, the mode in which any agent affects the bodily tissues and functions, but experience and experiment, induction and deduction, chemistry and physiology, enable us, in many cases, to arrive at the truth, or an approximation to it, and, consequently, there is at the present day a tolerably constant and unanimous belief respecting the *modus operandi* of a considerable number of dietetical and medicinal agents, and it is much to be lamented that two totally opposite and contradictory beliefs should exist on a subject of such vital importance to

the public as that of the action of alcoholic liquors upon the human body. I do not pretend to be able to settle this great and difficult question, but I shall endeavour, by quotations from high authorities, as well as by observations and reasonings of my own, to exhibit fully and clearly its present state, and thus to render its solution easier and less distant.

Alcohol appears to seek out and fix upon *nervous* matter and to act directly and specially upon it, just as other agents localize themselves in particular organs. Dr. Todd regards alcohol as a *food*—Dr. Carpenter as a *poison*—to nervous matter; both, however, agree that it acts upon the nerve-cell and fibre *directly*, and upon the encephalon almost to the exclusion of the spinal cord. "So far as it influences the nervous system," says Dr. Todd, "the action of alcohol is that of a *stimulant*—an unfortunate term, indicating a distinction without a difference; other forms of food are likewise stimulant, but as they do not act directly and quickly upon the nervous system, their exciting properties are not so apparent. In like manner, alcohol possesses its stimulating property, because it is a form of aliment appropriate to the *direct nourishment* of the nervous system, and to its preservation; its special adaptation to this system gives it an immediate exciting power superior to any other kind of food." He further says, that alcohol, even when taken in excess, does not produce inflammation of any organ, but that its bad effects are shown in the nervous system; it damages the nutrition of the nervous matter, poisons the nerve-fibre and nerve-cell, and produces anæmia of the brain. Waste of nervous matter is indicated by tremblings and impaired mental power, and these symptoms may be caused by mental anxiety, fatigue, or sexual excesses, with or without the use of alcohol. Todd contends that the moderate and proper use of alcohol repairs and invigorates the nervous system; in short, he regards it as "*its appropriate pabulum*," or food; if given beyond what is required in the treatment of disease, he says it will be exhaled and perceptible in the breath; not so if the quantity be proportioned to the wants of the system. Dr. Marcet, in a book, entitled 'Chronic Alcoholism,' has very well described the effects of the habitual and *excessive* use of alcohol. The symptoms, he says, may appear during the indulgence, or long after the discontinuance of the bad habit, and are these: Headache, vertigo, unsteady gait, tremors, muscæ volitantes, ringing in the ears, deficiency of what the French call "*aplomb*," nervousness, sleeplessness, nightmares, frightful dreams, weakness in the loins, hips, and knees, inability for exercise, and weakness of intellect and memory. The same or similar symptoms may arise from other causes besides alcohol; such as mental anxiety, excessive intellectual toil, sexual excesses, inordinate smoking, &c., all which, in common with alcoholic excesses, exhaust or *waste* the nervous matter. Dr. Todd, as I said before, regards alcohol as the proper pabulum of the nervous, in the same sense as albumen is the appropriate pabulum of the muscular, tissue. Carpenter, on the other hand

endeavours to show that alcohol is essentially *de*structive, and not *con*structive, of nervous matter; he contends that it only stimulates the brain and nerves as the spur does the horse, and that this stimulation wastes and destroys it, so that in the end, and on the whole, it will be in a worse condition than before, and will require time and repose in order that it may be fed and repaired by blood containing what he considers to be its appropriate pabulum—viz., phosphorized fats, albumen, &c. If every act of the mind, every thought and emotion, wears away some portion of nervous matter, just as every muscular contraction destroys a particle of muscular fibre, it is evident this must, in both cases, be restored in *some* way. Now Carpenter contends that alcohol cannot, and does not, restore nervous matter, but that it only stimulates the wasted and jaded brain and nerves to further efforts—that is, in fact, it acts upon them as the whip or spur does upon the jaded horse, making them work at the expense of still further wear and tear; so that alcohol, on this view, is a sort of suicidal instrument to the nervous system, goading it on to its own destruction! What, then, can and does repair nervous waste? Dr. Todd would answer, "Above all things, *alcohol*." Dr. Carpenter would reply, "Certainly *not alcohol*, but rest, sleep, and a blood containing the proper pabulum of nervous matter—fats, albumen, &c." No doubt there are many agents which all the world admits *do* repair nervous waste; such are tea, coffee, fresh air, recreations, sleep, and good food, about which there exists no sort of doubt or question; but good reasons ought to be given for excluding alcohol from the category. Now, of tea, coffee, exercise, study, sleep, &c., we may affirm, that used within certain limits, they stimulate, strengthen, nourish, and repair the nervous tissue; and that beyond those limits, they weaken, depress, and waste it. May this not be asserted, also, of alcohol? Dr. Carpenter argues that alcohol cannot ultimately benefit nervous matter, because it is incapable of regenerating it—i.e., of becoming its material pabulum, or food; but no one questions the benefit of tea, coffee, moderate study, sleep, and recreations; and yet we have no reason to suppose that they, *per se* and directly, contribute the most minute particle of matter to the brain and nerves.

In studying the physiological action of alcohol upon the human body, we must never forget that it is one of that large class of agents whose influence varies, not simply in amount, but in kind or quality, according to the quantity administered; so that the effects of a large dose will be, not a mere *multiple* of those of a small one, but of a totally different character. In some few cases, as those of lying or stealing for instance, quantitative difference does not produce qualitative difference; but in the majority of cases it does. A certain temperature produces ice—a higher one, steam; a certain weight bends a spring—a heavier one breaks it; a short mountain walk invigorates the body—a long one weakens it; a few hours' study may *in*nervate the brain—a few hours more will enervate it. And may not, also, a

certain amount of alcohol, tea, coffee, &c., strengthen the nervous system, and a larger one weaken it? Or is alcohol mischievous in *all* proportions, whilst tea, coffee, study, &c., are not so? Cause must be shown why alcohol is to be excluded from the class of agents which do good in moderation, and harm in excess.

Is alcohol a *food* or a *poison?* This question is still an open one; the end and aim of all food is *force:* food is finally converted into force, which may be regarded as its true definition, and almost as its equivalent, convertible, and equipollent term. In this sense, alcohol, tea, coffee, study, exercise, oxygen, may all be regarded as food, for they all give force, although they probably do not, directly and *per se*, furnish any material pabulum to the brain and nerves. Alcohol, however, *may* possibly do so, (as we shall see in another place,) and may, therefore, be regarded as a more real food than the others. Moderate exercise of mind and body is a generator of force, and the indirect means of imparting growth and strength to the brain and muscles. But alcohol is more really and strictly a food than any such agent as exercise; for all material food is either plastic (tissue making) or respiratory (heat-making), and alcohol is a most excellent respiratory or calorific food, for it is far more digestible, and far quicker in its action than starch, fats, and sugar, and is at once absorbed by the vessels on the walls of the stomach; consequently, where time is an object, as in cases of fainting, or of collapse from accidents, alcohol possesses a manifest advantage over the more solid and slowly-acting hydrocarbons. Even Carpenter admits the rapidity of its action; but he objects to its employment except in cases of emergency. He says—"Alcohol is the quickest in its action of all the hydro-carbons, but others would be equally and more permanently efficacious if only time were given them to act. In some exceptional cases this *time* cannot be given, and then alcohol is indicated." He also says, "Alcohol, by presenting itself first for combustion in the lungs, prevents the other carbonaceous matter of the blood (supplied from food and other sources) from being burnt off in the lungs; these, consequently, are thrown upon the skin, liver, and kidneys, which organs are very likely to suffer in the performance of this extra duty." All writers agree with Moleschott, that "wine saves the tissues from being burnt, by offering itself as fuel;" and the most recent experiments of the eminent physiological chemists of Germany have completely established this truth—that alcohol (in common with tea and some other agents), by preventing the waste of the tissues is, if not a real and material pabulum, at least an equivalent to it, a diminution of expenditure being, of course, tantamount to an increase of income. It is objected that alcohol is only a *temporary* stimulus; that the force generated by it is only *temporary*. But this is not a valid objection, since *all* stimulus, *all* force is temporary; food, fresh air, exercise, are all stimuli, or generators of force, but are temporary in their action. Life is only possible under incessant stimulus. Tea and coffee are

called "agreeable and refreshing" stimuli; why should the stimulus of alcohol be called noxious? What is there *peculiar* in the alcoholic stimulus that demarcates it from all the others? If a moderate quantity of tea be taken, the effect is agreeable and refreshing; but if taken in excess the effect is disagreeable and enervating. We may say the same precisely of a mountain walk; neither of these stimuli leaves the body in a worse condition (but, on the contrary, in a better) at the expiration of five or six hours than it would have been without it. Dr. Chambers applies this test (which seems to me a very fair one) to alcohol, and contends that if an individual finds himself better able to perform all the duties of life during the five or six hours' interval of his meals with than without alcoholic stimulants, then they are *good for him;* at the end of this interval, if alcoholic liquors agree with him, he will feel more cheerful and vigorous than he would have felt if he had not taken them. In the case of poisons, or *alcoholic excesses*, this average interval of five or six hours would be one of misery, and before its expiration the poison or the stimulus would demand imperatively either a remedy or a repetition.

All physiologists agree, that every mental effort wears away a portion of nervous matter, just as every muscular effort destroys and removes a particle of muscular fibre. This being so, the lost matter must, in both cases, be restored by the blood, unless a totally different law obtains in the two structures, which is improbable. Now, we know that the muscular tissue is repaired by the albuminous matters of the food existing in the blood. What part of the food, then, is it which, entering the blood, repairs, restores, or builds up the effete nervous matter? Phosphorus, oils, and fat constitute a large proportion of the nervous structures; and alcohol, being a hydro-carbon, is chemically allied to these components of nerve-cell and fibre. But what is its precise action upon them? Is it a real pabulum? Does it nourish them, directly and materially, and build them up in the same sense as albumen does the muscular tissue? Or does it merely affect them after the manner of study, or cheerful amusement, without imparting anything to them of a real and substantial character? Or, lastly, does it simply quicken the circulation, and so send a larger amount of blood to the brain and nerves in a given time? These and similar questions have yet to be answered before the subject of alcohol can be well understood.

My own observation and reflection have led me to believe that alcoholic drinks are highly useful, if not necessary, articles of regular daily consumption, for vast numbers of persons; but that their kind and amount must be determined by age, sex, constitution, mode of life, and other circumstances. I believe they are more necessary for those whose avocations involve head-work, anxiety, and wear and tear of brain, than for such as lead a comparatively animal life, or one of mere bodily labour. And I think it will be found that the degree of *refinement* of the alcoholic liquor required is in tolerably exact ratio

to the expenditure of brain-power. The agricultural labourer, for example, is satisfied with ginger-beer, or very poor home-brewed beer; the working classes of London with porter; clerks and shopkeepers with bitter ale; and barristers, judges, and members of Parliament with wine. In fact, we find a gradation of brain-work corresponding pretty exactly to that of the refinement and alcoholic power of the liquor habitually and instinctively made use of. On the continent, also, we see illustrations of the same fact—the strength and refinement of the wines consumed gradually rising with the exaltation of the brain-work of the consumers. Nor is this owing, as might be supposed, entirely to difference of rank or pecuniary resources; for every man finds the same fact illustrated and corroborated in his own experience. We all find, when on our tours in Switzerland or the Highlands, where we enjoy pure air, good food, and rest and recreation of brain; when, in short, we are living rather an animal than an intellectual life, we care nothing for, and do not require any sort of alcoholic liquor; whereas, when engaged in our professions or business in London, in the midst of bad air, noise, hurry, bustle, competition, and excitement, we are conscious of an unmistakable craving for a certain amount of alcohol with our daily food; the reason being that, in one case, we are doing everything to refresh and fortify, and in the other to exhaust and wear out the nervous system. This fact goes far to prove that alcohol, in some particular but as yet unexplained way, *does* repair nervous tissue.

In estimating the value of alcohol the experience and testimony of healthy persons who use it habitually, and in moderation, ought to be taken into account; also the fact that in all ages, and in every corner of the globe, man has discovered a method of preparing it. There are persons who do very well without alcohol; but this is no proof that it is useless to others. There are country districts where the labourers are healthy and strong without meat, and with beer almost as weak as water; but does it follow that the same fare would suit the London lawyer, barrister, judge, or member of Parliament? No, the two cases are totally different. Men whose labour resembles that of horses may and do live, like horses, upon corn and water; but those who are calculating, thinking, and reasoning twelve hours out of the twenty-four require a more refined sort of food and drink. A ploughboy will look fat and rosy upon his bread and cabbage and hard pudding and water; whilst a Gladstone will require, besides these, good animal food, tea, coffee, and an alcoholic liquor of great purity and refinement. If the brain-work of the London clerk demands a supply of Bass's ale, that of the working statesman will require something approaching œnanthic æther!

Two arguments used by total abstainers require a short notice. They maintain that alcoholic liquors cannot afford any real and permanent benefit because they contain little or nothing of a *solid* nature (as proved by evaporation to dryness). But if this proves the worth-

lessness of wine, so does it of tea and coffee! The fact is, experience has proved that all these agents, in spite of their unsubstantial nature, do refresh the wearied brain and nerves, and impart new life and health and spirits. Exercise, fresh air, recreations, study, tea, coffee, and cigar smoke, are all devoid of solidity; but the argument that they are *therefore* incapable of imparting anything to the human body is still more so. On the contrary, we know that exercise does add bulk and weight and substance to the muscles; that fresh air does redden and enrich the blood; that recreation and study do nourish the brain and nerves; that tea and coffee and alcohol do, at any rate, prevent waste of the tissues (and probably also directly nourish the nervous system); and that moderate smoking, by soothing and calming the over-busy and excited brain, prevents its exhaustion and waste; in short, some of the least material agents have the most real, powerful, and beneficial influence upon the human body. Again, teetotalers contend that in the case of alcohol it is impossible to define moderation and excess, since what is moderation to one man is excess to another, and *vice versâ;* but this is equally true of salt, sugar, tea, coffee, and many other things, moderation and excess in which they regard as tolerably well defined by common consent. The truth is, there is a certain recognised standard quantity of alcohol, salt, sugar, tea, coffee, &c., which all men agree to call *moderate*, and the difficulty is not greater in the case of alcohol than of any other article of daily consumption. The man who eats a leg of mutton at a meal, or consumes a pound of salt, or drinks a gallon of beer per diem, is looked upon by the public as a monstrosity, an exception, a wonder! whilst he whose daily consumption is one-sixteenth of these several articles is regarded as an ordinary individual—a type of the masses; in short, the excessive and the moderate man are as well known and as easily recognised as are any of the types and their deviations in the animal and vegetable world. It is idle and absurd to pretend that the boundary line between moderation and excess is indefinable. I believe every man knows where it is and when he has overstepped it, even although from long habit and blunted sensibility the transgression may have little effect upon him. The soldier's rations and the diet-lists of our great hospitals are so many proofs that *there is a standard* in these matters, well understood, and that public institutions, in their dietetical arrangements, do not contemplate or provide for monsters who eat a leg of mutton and drink three gallons of beer per diem!—*Lancet, May* 25, 1861, *p.* 507.

143.—ON VERATRUM VIRIDE.

By Dr. Otterson.

The veratrum viride is not the white or European hellebore, furnished by the mountainous districts of the continent; but is the American hellebore, and is found in swamps and low grounds from Maine to

Georgia; it is on this account sometimes designated swamp hellebore. In the United States Pharmacopœia and Dispensatory it is called veratrum viride, and by that name it is now familiarly recognised.

This article has never been used in the regular practice to any extent, although Prof. Tully, of Yale College, New Haven, had been in the habit of prescribing it in his practice, and recommending it in his lectures many years ago. In the August number of the 'American Journal of the Medical Sciences,' 1835, Dr. Charles Osgood, of Providence, R. I., published an account of his investigations and experiments with it, his attention having been called to it by Prof. Tully; still it received little or no attention from the profession at large, until the papers of Dr. Wm. C. Norwood, of Cokesbury, S. C., appeared about the year 1850.

That gentleman had been using it for several years, and his papers on the subject aroused the attention of the profession to what many practitioners, and myself among the number, considered a remedy of great importance as well as power. The best mode of prescribing the veratrum viride is in tincture; and as Dr. Norwood's preparation is now in the market, for the sake of uniformity, and while it is comparatively a new remedy, I should recommend that it be always prescribed. Its effects upon the system are generally prompt, certain, and of such power as not to be entirely void of danger. In its operation as well as in its nativity, the veratrum viride differs from the ver. alba., in that it does *not*, like the alb., produce catharsis, which is the principal operation of the latter; on the contrary, it is quite promptly emetic, and not cathartic at all. But its chief action, and that which gives to it its remedial value, is its wonderful control over the circulatory and nervous systems. It possesses in a pre-eminent degree the power of calming nervous excitement, and reducing the force and frequency of the heart's contractions.

It is indicated in all that class of sthenic diseases where prompt and persistent sedatives are required; and has the advantage over the lancet, in that its effects can be continued by slight repetitions, without reducing the vital powers, as would the same repetition of the lancet. It is preferable to digitalis, in that it is more prompt and certain, and has not yet been found to possess that cumulative property which constitutes one of the dangers of, and objections to, digitalis. It is preferable to antimony, in that it does not produce such violent enteritic irritation and griping diarrhœa, while as a sedative it is considered by many superior; and as an *expectorant*, fully equal to the antimony; its diaphoretic properties are about the same; in its local effects, it somewhat resembles this drug, as, when applied to the skin, it produces heat and irritation, though not such an eruption as will the antimony. The same may be said of its effects upon the alimentary canal. Hence it is contra-indicated when any gastric or enteritic irritation exists. I will now, in as few words as possible, introduce this new friend to a place in the society of those old and

long-tried ones, upon whose virtues we have been so long accustomed to rely. Nor have I any doubt that the addition will be found both pleasant and profitable.

I would recommend Norwood's tincture to be prescribed, in preference to Tilden's, or any other fluid extract, because I think it likely to be more uniform in strength, and hence in its effects.

It is adapted to all diseases requiring the action of the heart and arteries to be controlled, except, as above stated, those attended by gastro-enteritic irritation.

In the latter stages of disease, where the rapid action of the heart is consequent upon the progress of the disease, it should not be employed; such as the latter stages of fevers, when there are nervous symptoms and excessive cardiac action, attended with much prostration.

I think the doses recommended by Dr. Norwood, and some others, too large to commence with, particularly by those unaccustomed to prescribing it, and would rather advise them to commence with half the quantity, carefully observing its effects until they become familiar with them, and their own experience will be their most reliable counsellor.

I have used it myself in pneumonia, pleuritis, acute rheumatism, inflammatory croup, iritis, puerperal peritonitis, and nervous palpitation of the heart, and have occasionally added a few drops to expectorant mixtures, where the cough was produced or attended by bronchial irritation, which seems to be extended to the minute vesicles, and the expectoration difficult and albuminous in its character; and I must say, I consider it a most excellent addition; in its administration I have seldom commenced a case with more than three to four drops, repeating it in two hours, and then adding by one drop, at each repetition, or diminishing the quantities, and lengthening or shortening the interval as circumstances might demand. I have generally found a much less quantity to bring down the pulse as low as I required, than that mentioned by Norwood, and some others. When emesis is once produced, the pulse generally comes down sufficiently, the skin softens, or a profuse perspiration breaks out, and the great point is attained. This, as before stated, is easily continued by the occasional repetition of the remedy in very much diminished doses, varying, of course, as with all other medicines, according to the susceptibility of the patient. When emesis is once produced, I find two or three drops every hour or second hour control the symptoms which indicated its administration.

Reference has been made to the *power* of the veratrum viride. It sometimes occurs that its operation is active beyond anticipation, and excessive emesis and prostration, even it may be to an alarming degree, are produced. This may be immediately counteracted by the administration of stimulants; a little weak brandy and water with a few drops of laudanum in it, or a few spoonsful of camph. julep with

laudanum answer the purpose. I have never seen a case requiring carb. ammon., although it is advised. Sinapisms to the stomach are useful addenda to the treatment. In excessive emesis anodynes seem to be the antidote, and laudanum or syrup morphia are always at hand

I have made this imperfect and disjointed sketch, as at first stated, in order to elicit the experience of such as may have prescribed this remedy, and with a view to bring it to the attention of those who have not. Not that I consider our pharmacopœias deficient in the number of remedies they present; by no means; and yet I think new remedies of undoubted virtues should find there a place; notwithstanding they may be new to the great majority of our members. I presume the administration of this is new. At the South it is in extensive use, and highly prized, and by those who pretend to prescribe vegetable remedies only; it is at once in combination, their lancet, their digitalis, and their antimony; with them it is *tria juncta in uno.*—*American Medical Monthly Review, March*, 1861, *p.* 185.

144.—ON THE GALVANO-CAUSTIC APPARATUS.

By Dr. Thomas James Walker, Birmingham.

The galvano-caustic apparatus is extensively used in Vienna for the removal of morbid growths, as well as for other purposes. Owing to the absence, during the greater part of my stay last year in the Austrian metropolis, of Dr. Adolph Zsigmondy, *Primararzt* in the Hospital, who is the great advocate of the galvano-caustic mode of operating, I saw less of it than I should otherwise have done.

In the skin wards for the treatment of lupus, I several times witnessed its employment as a caustic by Professor Hebra. To all the parts affected, the instrument was applied at a white heat; thus, in a case of lupus of the ear, the porcelain cauteriser was deliberately applied to the whole surface of that organ. During the operation, which was necessarily a slow one, the size of the instrument being about that of an ordinary lead-pencil, the patient, a nervous female, continued shrieking violently. In another case, almost cured, the small pointed platinum cauteriser was applied to the numerous points where new centres of the inveterate inflammation were appearing upon the healed parts. During this process, the patient, a boy, scarcely uttered a groan; but it hard to say whether this depended on absence of suffering, or on the fortitude produced by the prospect of one or two *ten-kreutzer* pieces which the professor gave him for his good behaviour, when the operation was completed. Similar courage, or freedom from pain, was manifested by a young man in another case, where the cautery was applied round the edges of the several large patches of lupus non exedens, which existed about his body.

In Dr. Zsigmondy's practice, I had an opportunity of seeing the employment of this instrument, as a cautery to various ulcers of the leg. The galvano-caustic instrument in these cases, where the surface to be

treated was large, did not avail for the completion of the operation ; the aid of the ordinary actual cautery being called in, on account of the much larger heated surface which the iron heated in a charcoal fire affords, as compared with the small portion of platinum or porcelain which can be heated by the galvanic current. The first application of the glowing cautery to these parts appeared exquisitely painful, there being of course no particular virtue in the heat produced by electricity which should prevent its causing pain; but, in comparing the actual cautery as usually heated with that heated by the continuous voltaic current, as regards the pain caused by their application, we find that the latter, possessing in itself the source of heat, is not cooled by the tissues of the body, consequently it quickly and completely destroys their sensibility and vitality, producing only a momentary pain; while the former, however massive it may be made in order that it may retain its heat, is, on its first application to the tissues, so cooled by them, that it requires a longer application in order completely to destroy the structures with which it is in contact, and may in many places only half burn them, causing therefore pain of some duration. This I would advance as a general rule. To the eschar left where the instrument had been once applied in these cases of ulcer, it might immediately afterwards be reapplied without producing any pain. In pointing out what I conceive to be comparative merits of the galvanic cautery, I do not wish it to be supposed that I am advocating what appears to me an unnecessarily harsh and cruel treatment of a disease which does not require the use of the cautery at all. It may be that in gangrenous ulcers of the leg, and also in cases of hospital gangrene, many English surgeons would be inclined to adopt the treatment by the galvanic cautery; but for varicose and indolent ulcers, none of us, I think, would employ it.

In a case of large epulis growing from the outer side of the gum opposite the left wisdom tooth of the lower jaw, I saw the application of the cutting-noose *(Schneide-schlinge)*, an instrument which consists essentially of a platinum-wire ligature heated to a white heat by the galvanic battery, and capable of being gradually tightened by means of a screw. In this case, the wire noose being thrown round the tumour, and drawn so as to surround its base closely, the galvanic circle was closed, and the wire, becoming white hot, was slowly tightened; it gradually passed through the tissues until the tumour dropped into the mouth; to the part of the gum where it had been attached, the porcelain cautery was applied, and the operation was complete. As to the amount of pain caused by the removal of the growth, I am unable to form an opinion; for, although no chloroform was administered, the patient had fortified himself for the operation by drinking a quantity of wine sufficient to make him noisy and talkative. Very little coughing was caused by the smoke which the burning of the tissues produced in considerable quantity, the patient evidently respiring chiefly by the nostrils; the bleeding was very slight indeed, the patient spitting out

once or twice only a little burnt blood and saliva. The wooden spatulas which were employed to protect the tongue and the sides of the mouth did not quite answer their purpose, as the tubes containing the platinum wire, becoming heated during the operation, blistered both the tongue and the cheek. In other cases which I saw, the mode of proceeding was so much the same, that I will not detail them.

The galvano-caustic mode of operating has been brought to so much higher perfection in Germany than here or in France, and of the plan pursued by Mr. Marshall and other advocates of the proceeding in England so little is generally known, that I shall describe at some length the instruments employed by Dr. Zsigmondy in Vienna, which are almost the same as those employed by Professor Middeldorpf of Breslau, differing only in certain small points, which are, however, very important in their effect in improving the working of the apparatus. The essential part of the instrument is, of course, the galvanic battery; it is necessary that this be a constant one and very powerful. After many attempts to avail themselves of simpler and less expensive forms, the surgeons and instrument-makers have decided in favour of a Grove's battery as the most useful.

The communication between the battery and the instrument to be used must be made by means of wires sufficiently large to offer no opposition to the voltaic current, which must arrive in a full stream at the platinum wire which is to be heated; and these wires must at the same time be as light and flexible as possible, in order that they may interfere but little with the movements of the operator's hand. In Zsigmondy's apparatus, these conducting wires are formed thus. A bunch of capillary copper wires are carefully bound round with silk, so as to constitute a compound wire of about the twentieth of an inch in thickness; and from six to eight of these are again bound together to form a thicker wire. When the platinum wire of the instrument to be employed is very fine, so full a current is not required to heat it, and accordingly a lighter wire than that described may be used.

The instruments employed in these galvano-caustic operations are the various forms of cautery and the cutting noose, to most of which I have had occasion to allude. The various cauteries, as well as the cutting noose, are instruments in which a fine platinum wire, by being placed in communication with the two conducting wires, may be made the means of closing the galvanic circuit; these wires being made as good conductors as possible, and the far smaller platinum wire being a much worse conductor, the full stream of electricity meets here with such resistance as causes it to heat the wire to whiteness. The several varieties of cautery are formed on the same principle. They consist essentially of a wooden handle, through which run two copper wires, connected by their further extremities with the ends of the platinum wire of which the instrument is formed; while by the other extremities they may, by means of a screw, be made continuous with the conducting wires of the battery. The copper wires in the handle

are at least four times as large as the platinum wire of which the cautery is composed. One of them is cut across, and its two portions can be easily connected or disconnected by pushing a slide on the handle upwards or downwards, with the thumb of the hand holding the instrument; so that the combining disk being in its place and all ready for the operation, we still see the instrument cool and inactive, until the slide being pushed forward, the platinum wire begins to glow and rapidly rises to a white heat, at which it remains, when all the cells are in use and the wire is thin, even while in contact with the tissues to be acted upon. So intense is the heat that, unless the instrument be applied immediately to the tissues, so as to cool it somewhat, the wire will sometimes melt, high as is the melting-point of platinum.

The cauteries used are various, according to the platinum portion of the instrument. In the *Galvanokauter*, the platinum wire, one twenty-fifth of an inch thick, is flattened out into a form of blade with a beaked point; this is used as a cutting instrument, or its beak-shaped extremity as a fine cautery. In the porcelain cautery the platinum wire is spirally twisted round a small porcelain capsule, about the third of an inch in diameter; to this the heat developed in the wire is communicated, so that a much larger cauterising surface is obtained than could be got by the wire alone. Other forms of cautery for burning through urethral strictures, for passing down the lacrymal duct, for the destruction of the nerve-pulp of the tooth, &c., are also sold with the battery and other parts of this apparatus.

The cutting noose *(Schneideschlinge)* consists of a handle, from which projects forward a framework in which an ivory screw is fixed; round this the two ends of the platinum wire are wound, and when it is turned the wire is shortened. From the front of the frame pass forward two metallic tubes to contain the platinum wire; and these are isolated from one another by the ivory support; at the junction of these tubes with the frame between them and the handle are two screws, by which the platinum wire at this situation is made continuous with the conducting wires from the battery. The noose of platinum wire, which is usually about the fiftieth of an inch thick, being thrown round the part to be removed, the combining disk is placed on the battery, and the course of the galvanic force must be by the conducting wire to the screw at one side of the base of the double cannula; and, the communication of the wires below being prevented by a portion of ivory projecting round the middle of the screw mentioned, up the wire in the tube of the same side, round the loop, and down the other cannula to the screw at the other side of the base, and so back to the battery. Not only the projecting loop of platinum wire becomes heated, but also to a certain extent that within the tubes; and it is on this account necessary, in operating with the instrument at the bottom of the buccal, vaginal, or other cavities, to protect the neighbouring parts from contact with the cannula or with

the fluids which flow from the seat of operation very much heated; for unless this be done they will be burned, as in the case of epulis above described.—*British Med. Journal*, *April* 20, 1861, *p.* 409.

145.—ON THE INFLUENCE OF OZONE ON ORGANIC SUBSTANCES.

By Dr. T. K. Hornidge.

[The great differences in ozone from ordinary oxygen are all referable to its more energetic oxidising power. It is possible, and indeed probable, that ozone plays an important part in the chemico-vital changes of the body. The great source of ozone is unquestionably the electrical action unceasingly going on in nature.]

The ordinary action of ozone is one of energetic oxidation. In spite of the small proportion of ozone present, iodine or chlorine is readily set free from its combinations; silver leaf is converted into oxide of silver; even mercury is superficially oxidised; the white oxide of lead is converted into the brown peroxide; and certain noxious gases, as sulphuretted hydrogen, are completely destroyed, as may be readily shown by suspending in ozone a paper black with sulphide of lead: it is rapidly bleached, by conversion into the sulphate. Ozone, therefore, is the great natural scavenger of the air. Hence it is always most abundant where there is least impurity in the air. In mountainous regions, and by the sea, with a sea-wind, it is abundant; whereas near large towns, when the wind has passed over the town, it is wanting. Nevertheless, the relation of the presence and abundance of ozone to the presence of certain impurities in the air, especially in relation to the presence of the *materies morbi* of epidemics is enveloped hitherto in difficulties and uncertainty; due partly, I think, to the insufficiency of the ordinary test of ozone, which may be affected by many gases necessarily present in the air of large towns (as sulphurous or nitrous acids.)—*Med. Times and Gazette*, *April* 6, 1861, *p.* 357.

146.—FERRI CARBONAS EFFERVESCENS; A NEW AND ELEGANT FORM OF CHALYBEATE.

By Dr. Thomas Skinner, Obstetric Physician to the Liverpool Dispensaries.

Believing that the protocarbonate of iron, even in a solid state, is one of the best preparations we can administer in the thousand and one diseased conditions in which chalybeates prove useful, and that its present officinal preparations are incapable of preserving it from decomposition for any reasonable length of time, I have long thought it a desideratum to obtain it nascent, and in a soluble form, at the time of ingestion. I am now happy to state that after a great deal

of trouble and experimenting, both by myself and by various experienced chemists, I have at last succeeded in obtaining the protocarbonate in the permanent and elegant form of an effervescing granular powder.

Formula and Process of Preparation.—The effervescing carbonate of iron is made as follows:

℞. Acidi tartarici ℥iij; sodæ bicarbonatis ℥v; ferri sulphatis ℥x; pulveris sacchari ℥j ℥vj; acidi citrici ℥ij.

1. Mix the sulphate of iron with the sugar and part of the tartaric acid. 2. Mix the citric acid with the remainder of the tartaric acid and the bicarbonate of soda. 3. Add the mixtures, and thoroughly incorporate them by sifting. 4. The whole is now to be thrown into a metallic pan set in a water bath; in a few minutes it will separate, when it should be rapidly stirred until granules are formed. If preferred, it may then be flavoured with oil of lemon; hitherto, however, the preparation has been without it.

Physical and other Properties.—When the above is carefully prepared, it has all the appearance of the popular and well known granular effervescent citrate of magnesia, with the addition of a slight yellowish green tint. Every drachm and a half contains ten grains of sulphate of iron, which, with a complement of bicarbonate of soda, is certain to produce, in a state of solution, four grains of nascent protocarbonate of iron. At the same time, there is developed a tartrate with a little citrate and sulphate of soda, which is, if anything, an advantage, as they act the part of a very gentle saline aperient, obviating the usual astringent effect of preparations of iron, as well as the too frequent constipation attending cases requiring chalybeates, particularly amongst females. The taste of it depends very much upon the amount of dilution. When taken in the dose and manner hereinafter recommended, the taste is that of a mild, sparkling, and refreshing chalybeate.

After the effervescence subsides a perfectly clear light-green solution remains, which, if allowed to stand for some time, becomes of a deeper green colour on the surface, gradually increasing from above downwards, and floating like a cloud upon the upper stratum of the liquid. This appearance was at first mistaken for oxidation, but the more direct explanation seems to be that it is the carbonate of iron which was retained in solution by excess of carbonic acid gas; that, as the excess escapes from the surface, the carbonate separates from the solution in the form of a fine cloud, and becomes ultimately precipitated in the form of an impalpable powder. So far as permanency is concerned, the preparation has stood the test of several months, and it now remains as good as the day when it was made.

Dose, Uses, Administration, &c.—The dose is a teaspoonful, more or less (about a drachm or a drachm and a half), twice or thrice a day, in half a tumbler or more of water, an hour after, between meals, or upon an empty stomach, as is found most suitable. Dilution, within

reasonable bounds, increases the tolerance of the remedy and favours its therapeutic action. It may be drank during the action of effervescence, but it seems to me preferable immediately after it subsides. When a prolonged course of iron is required this preparation will never take the place of the protosulphate, the iodide, or the sesquichloride; but where a moderate course of a few days, or a week or two, is necessary, it will be well borne by the stomach. It is not only well borne, but it seems to produce a much more manifest chalybeate effect within a given time and in a smaller dose, than any other preparation of iron with which I have had experience.

In facial or other forms of neuralgia, arising from anæmia or other cause relievable by iron, and particularly if the bowels are at all torpid, a few doses often act like a specific. It ought, however, to be continued after the pain disappears, so as entirely to remove the condition upon which the neuralgia depends. I may remark that the quantity of iron and the aperient effect might be doubled if required; the present proportions, however, have been found by experience to be the best for ordinary purposes.

The pharmaceutist who has prepared this effervescent chalybeate for me, and who deserves great credit for the perfection to which he has brought it, is Mr. Banner, chemist, 4, Hardman-street, Liverpool.

Let me add, in conclusion, that in prescribing chalybeates in general, particularly for those who are fastidious about the colour of their teeth, I am in the habit of ordering at the same time the following wash, to be used with a tooth-brush morning and evening, or after each dose:—

R. Potassæ quadroxalatis ℥ss; aquæ rosæ ℥vj. Solve.

After the use of this the mouth should be rinsed with cold or tepid water.—*British Med. Journal, June* 15, 1861, *p.* 627.

147.—ON THE CHRONOMETRY OF LIFE AND DISEASE.

By Arthur de Noe' Walker, Esq., Paris.

That a child born in the seventh month is more likely to live than one born in the eighth, is assured as a fact throughout Europe. When I attended lectures, and heard this stated *ex cathedrâ*, the reason I adduced was, that a fœtus of seven months old, although immature, has at all events attained to the first stage of *relative* perfection; and that, therefore, the various forces and functions of its organism, although imperfect, are nevertheless, as a co-relative whole, in sufficient harmony with themselves to carry on the work required for further development. A child born in the eighth month is no longer in this *relatively* favourable condition; for after the seventh month a new series of progressive changes commence, perfected at the end of eight more hebdomads. Now, a child born while this new progressive stage is going on comes into the world in circumstances intrinsically

less favourable than one born in the seventh month, because a child born in the eighth has passed out of a relatively harmonious development into an imperfectly co-ordinate relation of parts. That which is imperfect, but whose imperfections harmonize, is better than one who, although more advanced toward ultimate perfection, is called upon to contend with the external world with an inharmonious adaptation of parts.

Septenary periods are especially remarkable among the Ovipara. I believe many of them sit on their eggs for seven days, or during some multiple of seven; a question immediately connected with the chronometry of life, beautifully seen even in the vegetable kingdom, wherein wonderful order is obtained in the midst of excessive complication of parts and functions. In some individuals, for example, as noted by Mr. Paget and others, each stamen at different periods bows down to the ovary, impregnates it, and then resumes its original posture, and makes way for another, whose organic development opportunely enables it to fulfil its destiny. It would be easy to ascertain if some particular part or parts of the chick are relatively perfected at the end of certain septenary periods. Human hair is apt to fall in greater quantity about the last septenary period of each lunar month, and to flourish again at the end of the second week, as I have more than once verified in my own person. Liebig states that wine in bottles undergoes a slight fermentation at the end of every month. It seems generally true that "to *every* thing there is a season, and a time to *every purpose*, under the sun;" and perhaps the chronometry of life is also subordinate to septenary or to other fixed periods.

The moral and physical phenomena observed in a healthy subject at the third septennial is particularly striking. Men at that age who are in the army and navy evince excessive daring and enterprise; while all assume a certain confident walk and tone, betokening intolerance of correction or contradiction in argument. We sometimes observe enormous energy displayed in furtherance of some favourite pursuit or chosen career. Advice, dogmatically given, is rejected with resentment; but sound counsel, offered in a proper and kind way, is gratefully received and seldom forgotten. This part of the subject reminds me of Burns's song—

"O for ane-and-twenty, Tam!"

I observe, however, that men at this age are apt to fail, if subject to great and prolonged privations and hardships; nevertheless, a regiment composed of equal proportions of young men and old soldiers is the best fighting material. At the end of the fourth septennial, a great change takes place. The mind sobers a good deal; the respiratory function is slower and less energetic, with a marked tendency to increase of bulk and adipose tissue. During the two following septennials a man is endowed with those qualities which render him most fitted to fulfil his individual responsibilities, and to give a per-

manent and useful character to his social position, inasmuch as he then possesses great mental and bodily strength, guided and governed by a mind free from the recklessness of twenty-one.

The chronometry of disease, although easily studied, has not been sufficiently investigated. I have lately turned my attention to this subject, but my observations are as yet too incomplete to be trustworthy. That septenary periods are often remarkable in the course of acute disease, is known to many nurses of common experience. In small-pox and after vaccination the papulæ and vesicles are matured on the seventh day, and visibly so on the eighth—a day in which a new phase commences in all cases where the seventh completes an antecedent one. Has any one ever observed whether relapses are liable to occur at stated periods? If so, perhaps some analogy might be traced between diseases liable to relapse at the same fixed periods.

While changes on the seventh day, or some multiple of seven, are generally unfavourable, it is, I believe, equally true that we may observe hopeful symptoms of amendment at fixed periods, varying according to the nature of the morbid state. I have myself remarked, in some fatal cases of diphtheria, a decided amelioration on the fifth day, increased tendency to death on the seventh, and, if the patient survived the first hebdomad, death ensued during or about the twenty-fourth hour of the fourteenth day after an antecedent amendment *perceptible* on the twelfth. I have never been able to trace any kind of septenary change after the twenty-first day.

Mr. Rice's remarks on the general question of sympathy are sound. Sympathy and antipathy stay society to an almost inconceivable extent, and, like instinct, are independent of reason, merit, or prejudice. But observation will prove the truth of Mr. Rice's assertion, that our antipathy or sympathy for a person is due to individual organisation, and has, therefore, like all other phenomena, a material basis. His remarks, also, on "reflex motion" commend themselves. Moral reflex action—a totally distinct thing from example—exercises immense influence over mankind, especially in civilized cities, or where large civilized masses consort together. Moral reflex action is nothing more, however, than sympathy induced by more active means than is the case in what Mr. Rice calls "hidden sympathy," *apparently* quite passive. The former, however, operates very powerfully, either for good or for evil on the great mass of the people; a fact, perhaps, so general, that it seems to have escaped the notice of most statesmen in Europe, save in Austria and Russia, where due attention to this subject is a great help to despotic governments.

With reference, however, to Mr. Rice's remarks, I will observe that the present Lord Radstock has propounded the following theory: He believes that when a public speaker or preacher, for example, influences his audience to an unusual and often unhealthy extent, and, as the saying is, "carries them with him" such sympathy, or moral reflex action, is due to the speaker's ability or power of exciting in

each individual before him that particular part of the main nervous centre peculiarly excited in himself while addressing his audience; or, in other words, every person present has the same identical part of his brain aroused or excited. This explains why, as I have myself seen, a whole division of an army may be overtaken by a frightful panic, developed in the first instance in half a-dozen men, or in a single advanced sentry. In the same way we may explain why a single hysterical patient may propagate hysteria to the inmates of a whole ward. St. Vitus's dance also is propagated in the same way, by one person so diseased putting the affected part of his own nervous system *en rapport* with others; and thus it is, I believe, that a few persons have infected whole villages. I have observed, and I believe, verified this theory at "Revival" meetings; and if so, the material origin or medium of similar reflex actions is satisfactorily explained.

I have always thought the phenomena connected with panics very extraordinary. Troops that have faced death every day, and ready to do so oftener if required, will fly across the country "panic-struck." Monetary panics are not unfrequent, and "a run" on a savings bank sometimes takes place without any real or apparent cause. An "invasion panic" may be induced by the public press in forty-eight hours. Panics of a more reasonable nature are apt to pervade whole masses, from slight shocks of an earthquake, or from a sudden outbreak of an epidemic. Liability to panic-terrors being universally prevalent among all people, the ancient sacerdotal craft attributed the cause to a god, and assigned the attribute to universal Pan.

Sometimes, and in particular circumstances, the results of these reflexed influences are singularly permanent. I verify this in the person of a near relative of mine. A few years after her marriage, while out riding with her husband, his horse reared, fell over him, and killed him. Ten years after, being still a young woman, she married again, and bore children; but all the males, and in one case painfully so, unmistakably resemble her first husband. A renowned English physician, one of the most trustworthy observers that ever lived, known and quoted in all the schools in Europe, declared that convalescence in old men is much abbreviated, and the whole organism strengthened, by consorting, and even sleeping, with very young persons of the same sex.

The majority, of course, only smile at all this. Those whose only aim in life is to "get on in the world" declare these questions useless, *even if true;* while a smaller number, who have already "got on," don't care about it. There is, however, a very small minority who love their noble profession for its own sake and for the sake of others; and for them I close these remarks with a quotation from the works of the most learned man that ever sat in Parliament: "We would introduce into primary philosophy a real and solid inquiry into these transcendentals or adventitious conditions of beings, according to the laws of nature, *not of speech.*" (Bacon.)—*Medical Circular, July 24, 1861, p. 65.*

118.—ON INTERNAL OBSTRUCTION BY BANDS.

By John Gay, Esq.

(Abstract of a paper read before the Medical Society of London.)

The records of cases in which the abdomen has been cut into for the purpose of relieving internal obstruction, upon the presumption that the symptoms were due not only to a physical, but to a removable agent, show that the difficulty of distinguishing its exact nature and seat in each case has not been overcome. I need not refer to the recorded cases of Manoury, Dupuytren, Monod, Hilton, Druitt, Avery, Fergusson, Erichsen, Recamier, Curling, Luke, Holt, and my own. In some of these the seat of stricture could not be discovered; in others the stricture was discovered to be occasioned by irremediable or irremovable disease; whilst in a third class the state of the parts was anticipated, but relief was too long deferred. It is true that in a few cases, such as those recorded by Fuschius, Caryoche, Dr. Wilson, Dr. Manlove, and Schacht, success followed these operations: but, on the whole, the best surgeons have deemed them speculative; and a lull in this species of abdominal surgery chides us for the neglect of those investigations which may render our knowledge more exact, and our treatment consequently more useful.

Of the various causes of internal obstruction which come under our notice, I have chosen the "band" for present consideration. And here I must premise that by the term "band," I mean a structure distinct from ordinary adhesions, and having in its origin, texture, and relations, characteristics peculiarly its own. As these bands are generally limited in each individual—seldom exceeding one, and never, as far as I have been able to discover, more than two,—I shall take the liberty of applying to it the generic term "solitary," in contradistinction to the filamentous bands that are often met with in the abdominal cavity, and which are only a variety of the common plastic exudations of its serous membrane. From these it differs, 1st. in size; 2ndly, in the fact that it is rarely if ever attached but at its extremities. The former are attached throughout their length to that which they embrace or connect—the latter *never*, their only attachments being *terminal*. 3rdly, in their having a distinct glistening surface, as though they possessed, in common with the abdominal viscera, a peritoneal investment. 4thly, in the fact that they are (I believe) frequently found to be *tubular, when they stretch from one hollow viscus to another*, and form a channel of communication between them. And 5thly, inasmuch as they have always some definite length. At the time of their formation, the parts connected by these "bands" appear to have been in *abnormal relation* to each other, whilst in simple plastic exudations the parts appear not to have been displaced.

Let us now look more narrowly into the ordinary conditions and

relations of the "solitary" band. It has mostly *two* attachments—viz., *terminal*,—and sometimes, though rarely, it has but *one*, being free to float amongst the abdominal viscera.

The classification given in the former part of the paper shows that, of the total number of bands, 27 were attached by one or both ends to the mesentery; that they may be formed upon and have their terminal attachments to that structure: but that when attached by one end only to it, their opposite extremities have either an intestinal attachment, as was the case in *twelve* instances; are attached to some contiguous organ, as in three instances; or it may be *free*, as in one. If we take another view, we shall find that when attached on one side to the bowel, the band becomes attached on the opposite, besides the mesentery, either to another portion of intestine, as in ten instances (including in these the vermiform appendage of the cœcum, which was the point of attachment in five cases)—or to a neighbouring viscus, as in three—or to the parietal peritoneum, as in two.

The general inference from these facts is, I think, that whatever the terminal attachment of these bands, one attachment at least is always to some *one* of those abdominal structures or viscera, which are very prone to become, at some period of life, the seat of serious and destructive diseases; and that, in all probability, these bands differ in their origin from those abnormal products with which they have been allied, inasmuch as they are the productions of the gravest disorders to which the abdominal viscera and their appendages are liable. And the view just enunciated becomes, I think, strengthened by an examination of the comparative liability of different abdominal structures to become the seat of bands, and the seat of destructive disease! A band of varying length—in some instances over 17 inches—may seem at first sight a very unlikely provision for the care of a wound or an ulcer through the intestinal walls; but the improbability fades away before such facts as I am now about to lay before you.

1st. Traumatic injuries of the bowels are frequently remedied by the formation of a band; tying the bowel, at the seat of injury, to the tissue which happened coincidently to be in contact with it.

2nd. Morbid specimens abound in which the connection of a band with an intestinal cicatrix is shown.

These affections arise—

I. From impactions of hard bodies—such as gall-stones, plum-stones, &c. &c.—in the bowels or certain diverticula, especially in the vermiform appendage.

II. From the ordinary destructive disease of the abdomen,—mesenteric tabes, idiopathic typhlitis, typhus, catarrhal and dysenteric diarrhœa, &c.

III. From sloughing of portions of the intestinal cylinder, in consequence of injury done by external strangulation, as through hernia.[5]

3rd. The retention of a viscus, in an abnormal position, for a *longer or*

shorter period (for it is impossible to assign any *proximately* fixed time for the formation of bands of this class), will give rise to the production of a band, in all probability, in consequence of direct or indirect injury during the period of dislocation. The most common instances of this kind of "band" are those in which a portion of perviated bowel becomes attached to the *sac*, in which it has been allowed to remain unreduced for some time. Such bands not unfrequently cause the reappearance of the hernia after repeated *reductions*, and might always be suspected when it assumes so *obstinate* a character.

But to proceed. *These bands*, it must be remembered, do not always constitute the whole of the immediately constricting agent, *bridle*, or *ring;* they may not constitute even any part of it, although that agent could not have existed as such without them. Thus a twisted and elongated *mesentery* may constrict through the adhesion of its *bowel* to some other structure. The omentum might also constitute the constricting agent through a like attachment, as in cases in which it has thus been tied to the edge of the femoral ring. Even a portion of intestine, or its diverticulum, might, in a similar manner, become the immediate *agent* in the act of constriction. In one case a band so attached a knuckle of intestine, that it (the knuckle) formed a ring, and strangulated another portion of intestine that happened to pass through it.

The evidences of the existence of a band may be divided into those derived from, 1st, antecedent history; 2nd, mode of attack; sensatorial, 4th, physical, and, 5th, functional indications.

(Here follows an *analysis* of these several evidences, derived from a large number of collected cases.)

Perhaps this survey of the means of diagnosis is somewhat *discouraging*. It amounts to this:

1st. The history of the individual should point to some antecedent disease or injury equal to the production of a "solitary band."

2nd. Pain does *not of necessity* immediately follow on the constriction of a portion of intestine by a band, although a portion of higher intestine be constricted, and that tightly. If it *is* coincident with the *first* symptoms, it is *local*, and arises from a physical injury to the bowel; but if not, it comes on sooner or later, as the consequence of *over-distension*, or of *peritonitis* and its results, or of *both* combined. That it is not always, but *often paroxysmal*.

3rd. That distension is an early and prevailing sign. When the constriction involves the higher bowel, it is generally confined to a limited area around the seat of injury; when it is lower it is more *general* from the commencement. Dulness is a sign of uncertain value; but in general it is found gradually to concentrate itself, together with greater distension, towards and around the seat of constriction. Vomiting is a prevailing symptom, and especially characteristic of complete obstruction when it becomes fæcal.

4th. The suddenness of the attack, and its not being preceded or

accompanied by any indications of any alteration in the prevailing state of the patient's health.

The practical conclusion at which I arrive from the foregoing remarks is this: viz, that our present means of diagnosing internal obstruction by *bands* are imperfect, and that, consequently, many cases will escape the most careful scrutiny to ascertain their exact cause. On the other hand, the inductions from the confessedly feeble analysis of the cases which I have collected together, and thus cursorily examined, encourage and even authorise the surgeon, in a case presenting a certain combination of features or evidences, rather to advise abdominal section than allow the patient to sink without the chance that such a procedure might afford.

1st. An *antecedent abdominal affection* of such *severity* as to lead the surgeon to believe that it might have been attended with some destructive process.

2nd. The attack occurring *suddenly*, without any previous visible deterioration of the patient's health.

3rd. *Pain first localised*, then tenderness over a large area.

4th. Distension, with, *first*, general dulness, and subsequently a concentration of the dulness and tension towards the original seat of pain.

5th. Vomiting, especially if it speedily becomes fæcal.

I believe no such conjunction of symptoms as these can arise without some *sudden* alteration in the relation of parts within the abdominal cavity; and that in such cases a *bridle* will be found to be, either directly or indirectly, the means of constricting a portion of intestine. As soon as the last of this series—viz., fæcal vomiting—has set in, the surgeon is justified in proceeding to explore the abdomen. Should a band or bridle be found, some caution must be observed in its treatment. It should be divided; but in case it unites two portions of a hollow viscus, there is a probability of its being tubular: consequently, the two ends should be twisted or tied to insure of their not allowing the escape of the visceral contents.—*Medical Circular, April* 3, 1861, *p.* 235.

149.—CRINOLINE ACCIDENTS.

By Dr. Odling, F.R.S., Guy's Hospital.

The various means proposed for rendering textile fabics non-inflammable were carefully investigated a short time back by two well-known chemists, Messrs Versmann and Oppenheim. An account of their experiments was read at the Aberdeen meeting of the British Association in 1859, and was afterwards published in the 'Journal of the Society of Arts,' and in a separate form by Trübner and Co., of Paternoster-row.

They showed that linen and cotton goods dried after immersion in a

solution of one or other of several salts possessed the property of non-inflammability, and that the best results were obtained with a solution of sulphate of ammonia, or of tungstate of soda, neither of which liquids produced any injurious effect upon the tissue or colour of the fabric. The tungstate of soda solution was found most applicable to laundry purposes, on account of its not interfering in any way with the process of ironing.

Muslins, &c., steeped in a 7 pr cent. solution of sulphate of ammonia, or a 20 per cent. solution of tungstate of soda, and then dried, may be held in the flame of a candle or gas lamp without taking fire. That portion of the stuff in contact with the light becomes charred and destroyed, but it does not inflame, and consequently the burning state does not spread to the rest of the material.

I am induced to send you these remarks through the mention of my name by Dr. Gull at the recent inquest on the death of Mrs. Brodhurst, as reported in the 'Times'.—*Times, July* 29, 1861.

150.—EXPERIMENTS RELATING TO THE DIURETIC ACTION OF COLCHICUM.

By Dr. William A. Hammond, Professor of Anatomy and Physiology in the University of Maryland.

In the Proceedings of the Academy of Natural Sciences of Philadelphia for November, 1858, I gave the results of a series of investigations relative to the diuretic properties of digitalis, juniper, squill, and colchicum, by which it was shown that the latter alone possesses the power of increasing the amount of organic matter eliminated by the kidneys. From this circumstance the argument was adduced that this substance, of all those experimented with, was the only one that could be regarded as a true depurator of the blood.

The results obtained by earlier investigators cannot be regarded as satisfactory, owing to the faulty manner in which their analyses were made. The urine was concentrated by heat, and thus a large quantity of its organic matter underwent decomposition.

Since the publication of my experiments, Dr. Garrod of London has studied the physiological action of colchicum; but led away by his theory of the nature of gout, he limited his researches mainly to the termination of its influence over the excretion of uric acid, which, as is well known, forms but a small proportion of the total amount of organic matter excreted by the kidneys. As the result of his investigations, he announced that colchicum does not increase the quantity of uric acid contained in the urine, and that it is not by any action on the kidneys that the remedy in question exerts its curative influence in gout. His result, as relates to the uric acid, does not, so far as I know, conflict with mine, as I did not separately determine the quantity of this substance present; but his conclusion that colchicum is not a diuretic in the true sense of the term, is certainly not borne

out by his own experiments, and is directly at variance with those which I performed.

It was, therefore, obviously necessary that additional investigations should be instituted, and I accordingly undertook the task of furnishing further contributions to the subject. Before proceeding to detail these, I desire to call attention to the valuable memoir of Prof. Austin Flint, in the number of this journal for November, 1860, entitled, 'Clinical Researches on the Action of Diuretic Remedies.' In this essay, in addition to much other valuable matter, the conclusion at which I had arrived relative to the action of colchicum is confirmed; Professor Flint finding it to produce a marked increase in the amount of solid matter eliminated by the kidneys, without, however, increasing the quantity of water of the urine.

The investigations to which the present paper relates consisted of experiments upon adult males, in a good condition of health. In all cases, the officinal tincture of the seeds of the *colchicum autumnale* was given.

The determinations made were the following:—1st, the quantity of urine; 2nd, its specific gravity; 3d, the total amount of solid matter; 4th, the quantity of inorganic matter; 5th, the quantity of organic matter; 6th, the amount of uric acid.

The quantity of urine was determined in cubic centimetres.

The specific gravity was ascertained by means of the specific gravity bottle and a delicate balance.

The total amount of solid matter is given in grammes, and was determined in the following manner:—Ten cubic centimetres of the urine were evaporated to as complete dryness as possible *in vacuo* over sulphuric acid, and the residue accurately weighed. By simple proportion the amount of solids in the whole quantity of urine was easily ascertained.

Although it is impossible to get rid of all the water by this process, the quantity remaining is extremely small, and the results obtained are far more accurate than those obtained by evaporating to dryness in the water-bath, as generally practised. No matter how carefully this latter process is conducted, the loss of urea by decomposition is always an important item, and involves far more serious errors than the imperfect desiccation by the former process.

For the determination of the amount of organic and inorganic matter separately, the solid residue obtained as above was mixed with ten or fifteen drops of moderately strong nitric acid, and gently heated till the mass was well dried. The heat was then gradually raised till all the carbon was consumed, and the mass in consequence became white. It was then cooled *in vacuo* over sulphuric acid, and weighed. The inorganic matter was thus determined, and the loss showed the proportion of organic substance.

The quantity of uric acid was determined by adding chlorohydric acid to a known volume of urine.

The first experiments were instituted upon myself. In three days immediately preceding their commencement, the average quantity of urine for each day was 1425 cubic centimetres. of specific gravity 1021·73. The average amount of solid matter was 64·28 grammes; of which 30·18 were inorganic, and 34·10 organic substance. The average amount of uric acid excreted for each period of twenty-four hours was 0·77 gramme.

During the experiments with the colchicum, my manner of living was not materially altered from that of the three days above referred to; *i.e.*, I ate the same food and took the same amount of exercise, and endeavoured to make all the collateral circumstances the same, so as to ascertain as nearly as possible the exact effect produced by the colchicum.

First Day.—On this day I took one fluid drachm of the tincture three times—at 8 a.m., 2 p.m., and 10 p.m. The total quantity of urine excreted was 1685 cubic centimetres, of which the specific gravity was 1021·50. The total amount of solids was 70·15 grammes, of which 30·90 were represented by inorganic, and 39·25 by organic matter. The quantity of uric acid was 0·81 gramme.

Second Day.—One and a half fluid drachms of the tincture were taken, as on the previous day. Quantity of urine, 1720 cubic centimetres; specific gravity, 1020·87; total solids, 75·29 grammes; inorganic solids, 32·44 grammes; organic solids, 42·85 grammes; uric acid, 0·69 gramme.

Third Day.—Same quantity of colchicum taken as on previous day. Quantity of urine, 1784 cubic centimetres; specific gravity, 1022·57; total solids, 80·13 grammes; inorganic solids, 35·11 grammes; organic solids, 45 03 grammes; uric acid, 0·82 gramme.

Fourth Day.—On this day the quantity of colchicum was reduced to half a fluid drachm, taken as before. Quantity of urine, 1540 cubic centimetres; specific gravity, 1023·17; total solids, 69·23 grammes; inorganic solids, 31·09; organic solids, 38·14 grammes; uric acid, 0·78 gramme.

Fifth Day.—On this day the quantity of colchicum was increased to one and a half fluid drachms of the tincture before mentioned. Quantity of urine, 1698 cubic centimetres; specific gravity, 1023·68; total solids, 76·14 grammes; inorganic solids, 32·26 grammes; organic solids, 42·88 grammes; uric acid, 0·76 gramme. On this day there was some derangement of the general health, manifested by increased heat of skin, fever, and severe abdominal pains. There was also a little diarrhœa. The experiments were, therefore discontinued.

From an examination of the results obtained by the foregoing investigations, the effect of the colchicum upon the urinary excretion cannot fail to be perceived. The conclusions which I think may be formed are—1st, That the colchicum increases the quantity of urine. 2nd, That it increases the total amount of solid matter eliminated. 3rd, That this increase is mainly due to an augmentation of the

organic matter. 4th, That the amount of uric acid does not appear to be affected.

These conclusions are rendered much more probable from the fact that on the fourth day, when the quantity of the tincture of colchicum taken was reduced one-third, the effect upon the urine was less decidedly marked; and that when, on the fifth day, it was again augmented to a drachm and a half, the urinary excretion was materially increased in quantity, and the solids, the organic especially, remarkably raised in amount. The relation of cause and effect would therefore appear to exist; and accordingly it would be contrary to the principles of sound reasoning to assert that the change in the composition of the urine was accidental. It is doubtless true that the urine changes greatly from day to day, and even from hour to hour; but this fact is due to the other fact, that we are constantly varying our food, exercise, &c. When, however, as in the investigations cited in this paper, these circumstances are fixed, and only one difference exists between the ordinary mode of living and that practised during the continuance of the experiments, we are fairly justified in attributing any change in the urine or in any other excretion to the influence produced by that difference.

In the next series of experiments the effect is just as directly shown, though, for reasons beyond my control, they were not continued as long as were desirable.

The subject of these experiments was a young man 23 years of age, and weighing about 140 lbs. Before taking the colchicum I examined his urine whilst he was taking a fixed quantity of food and exercise, he being at the time an attendant in the hospital under my charge. As the results of these examinations for three consecutive days I obtained the following as the averages for each day:—Quantity of urine, 989 cubic centimetres; specific gravity, 1020·14; total solids, 51·20 grammes; inorganic solids, 22·45; organic solids, 28·75; uric acid, 0·47 gramme.

First Day.—On this day one drachm of the tincture of colchicum was taken three times. The effect upon the urine was as follows:—Quantity, 1021 cubic centimetres; specific gravity, 1024·18; total solids, 63·25 grammes; inorganic solids, 23·57; organic solids, 40·68 grammes; uric acid, 0·59 gramme.

Second Day.—One and a half drachms of the tincture were taken three times, as previously. Quantity of urine, 875 cubic centimetres; specific gravity, 1026·11; total solids, 60·25 grammes; inorganic solids, 20·38 grammes; organic solids, 39·87 grammes; uric acid, 0·51 gramme.

On this day diarrhœa was produced. This was quite of a severe character, and in consequence the colchicum was not further continued.

The remarkable effect of the colchicum in increasing the amount of organic matter excreted is, however, very decidedly shown. This increase is so great as to render the probability of its being accidental

extremely small, and we cannot do otherwise than regard it as being directly due to the influence of the colchicum.

The details of the third case in which the colchicum was given have been unfortunately mislaid. I am, however, enabled to state with certainty that the same well-marked effect over the amount of organic matter excreted by the kidneys was exerted as in the cases the particulars of which have been given in full. The experiments were continued for six days with variable quantities of the tincture.

What are we to infer from these investigations? It appears to me that the conclusion must be admitted that colchicum is a true depurator of the blood, and hence we have an explanation of its good effects in those blood diseases, gout and rheumatism.

It is seen that no constant effect was produced upon the quantity of uric acid eliminated, and hence these experiments do not conflict with those of Dr. Garrod. We are not, however, bound to admit that the presence of uric acid in the blood in increased amount, during a paroxysm of gout or rheumatism, is the cause of that paroxysm; and, consequently, because colchicum does not increase the quantity of this substance found in the urine, we are not to suppose that the remedy in question does not exert its influence through the kidneys.—*American Medical Monthly.—Glasgow Medical Journal, July*, 1861, *p.* 247.

151.–THE HOT BATH: ITS PHYSIOLOGY, USE, AND ABUSE:

ESPECIALLY IN CONNEXION WITH THE TREATMENT OF DROWNING.

By CHARLES HUNTER, Esq., late House Surgeon to St. George's Hospital.

"From these facts it appears that warmth causes a greater exertion of the living powers than cold, and that an animal in a weakly state may be obliged by it to exert a quantity of the action of life, sufficient to destroy the very powers themselves. . . . . The first (artificial respiration) in many cases may succeed alone; but the second (heat) must, I think, always fail where the powers of life are considerably weakened."—JOHN HUNTER *on Proposals for the Recovery of Persons apparently Drowned*, "Animal Economy," 2nd Edit., 4to, pp. 138, 139.

"Therefore in every case of vital suspension, the *primary object* is to institute *artificial respiration*, till the *natural breathing* can be re-established. . . . . During this, if we attempt to raise the heat suddenly to the natural standard, we exhaust the feeble remains of life.

"The most efficacious method of restoring heat, then, is to renew the generating power by renewing respiration."—Dr. A FOTHERGILL *on Drowning*, *in* "A New Inquiry into the Suspension of Vital Action," 1794, pp. 112, 134-5.

"On this account, and particularly as it is removing the cause of death, we cannot hesitate one moment in pronouncing the restoring the action of the lungs to be of the very first importance to recover the apparently dead."—CHARLES KITE, "An Essay on the Recovery of the Apparently Dead," 1788, p. 138.

"Thus we have traced the connexion of the symptoms, and the consequent cessation of the heart, to the obstruction of respiration, as to their proper cause."—EDMUND GOODWIN, "On the Connexion of Life with Respiration, Submersion," &c., 1788, p. 93.

"Death from drowning is similar to death from strangulation, and the want of the due oxygenation or decarbonization of the blood is the sole cause of the animal's destruction."—BRODIE'S "Lectures on Pathology," 1846, p. 86.

"It is surely not less essential to the progress of science and our art, to remove error than to establish truth. . . . . So that the warm bath is not only positively injurious by *poisoning*, but negatively, by excluding the de-poisoning process."—MARSHALL HALL, "On Drowning," &c., 1857, pp. 76, 78.

[Mr. Hunter, after citing the above quotations, goes on to observe that the arrest of cardiac action is *not* the cause of death. Dr. Waters of Liverpool, has shown by experiments that cardiac action goes on for an average period of nineteen minutes, dating from the commencement of asphyxia. The following inference may be drawn.]

That the longer cardiac pulsations continue, after the lungs have ceased to act and purify the blood, the more effectually is the system being narcotized by the non-eliminated carbonic acid in the blood.

It would be well for the chief surgeons of the Royal Humane Society to ponder on these important truths: that the lungs do cease the first to act; and that *the longer the heart acts*, either naturally or *by hot-bath stimulation, before the respiration is restored*, the more surely is the system being *poisoned* by carbonic acid narcotism. But they may possibly say—for they cannot argue the point physiologically—that the body is quite cold, the animal heat deficient; and that for these reasons, as well as to restore the heart's action, do they recommend the hot bath. There does, at first sight, seem some reason to justify the hot bath, not for the sake of cardiac stimulation, but to warm the body and retain what animal heat there still may be; but when the cause of the loss of animal heat is considered, I believe that true, sound physiology would discard the hot bath as a means of adding warmth to the body, and for retaining animal heat.

Indications for treatment.—What should be the rational treatment for a person with suspended animation, whose every symptom is traceable to deprivation of air and cessation of lung action?

"If a poor creature," says Marshall Hall, "be perishing for want of food, we cautiously administer food. If a man be in like manner perishing for want of air, should we not administer air? Is not this simple and reasonable? And, in the case of drowning, is not the want of air the first condition to which we should bring succour, and the want of temperature the second or third? And should we not first administer to the first want?"

Air is, then, the first want, and the power to breathe it the second.

What can be more clear or reasonable than the following admirable passages from Copland:—

"The indications are, firstly, to remove the patient as soon as possible from the causes which occasioned the asphyxied state; and secondly, to restore the functions of the respiration, and *through it the circulation*. The restoration of the function of the respiration is to be attempted by various means calculated, in the first place, to dislodge the impure air contained in the lungs; secondly, to replace it with pure air; thirdly, to excite the remaining vitality of the nerves and

muscles; and, fourthly, to restore the circulation by measures calculated to return the blood from the lungs to the left side of the heart."

Dr. Richardson's first conclusion from his numerous experiments on resuscitation is in accordance with the views of Hunter, Fothergill, &c. "In restoring animation," he says, "artificial respiration *should always be first resorted to*, in the hope that there may be still some remaining action of the heart."

To attempt at once, in suspended animation, to restore the respiration, is, then, the unanimous opinion of the highest authorities on the subject of drowning.

To attempt to renew the circulation in the first place is to act contrary to sound sense and true physiology. But by *first restoring the respiration*, we physiologically, *and in the proper order*, nullify the various evils shown in the above arrangement to be produced by submersion: we cause air to enter the lungs; carbonic acid to be exhaled; oxygen to be received; the expansive motion of the lungs to be resumed—the muscular respiratory action also; the blood to circulate through the lungs, and if it did still circulate, to be circulated in a purer state.

Then as to the heart—The establishment of the respiration unloads its right side of the excess of black blood accumulating there, and causes the left side to receive accessions of blood *gradually purer in quality*.

The system generally becomes more vitalized or gradually decarbonized. The brain and cord gradually resume their functions, consciousness and reflex action returning; a fresh and healthy impulse is given to the whole capillary system, in which "an arrest of blood change" had progressed to a greater or less extent; and by the removal of this stagnation of impure blood, one great (if not the) cause of the loss of animal heat is also removed.

By thus first restoring artificial respiration, we remove the *narcotism* affecting all parts of the system, or, as Sir B. Brodie expresses it "the want of the due oxygenation or decarbonization of the blood, the sole cause of the animal's destruction."

"Then, in the case of drowning, we should," with Marshall Hall, "administer air first, and warmth in the second place. But may not the warmth administered without air do great and absolute injury?"

Who can now imagine the drowned individual—whose every symptom is recognised by John Hunter, Brodie, Marshall Hall, and all other chief writers on the subject, to be traceable to the deprivation of air whilst in the water, and when out of it to the want either of the power to respire, or to impure blood from want of air—receiving benefit by the continuous hot bath? No plea can now be urged, unless contrary to all physiology, in favour of the hot bath, save for the warmth that it furnishes; and this warmth can be more rationally applied, when it is required, by other and more handy means.

The experiments previously related, in which I was kindly assisted

by Mr. Tomlinson, Mr. Braine, and other gentlemen, showed the "fatal tendency" of the hot bath: how that the lungs of animals drowned in hot water were more congested than of those drowned in cold; how the normal effects of a hot bath were to stimulate cardiac action for the first twenty minutes at least; and the still more important point that, whilst the heat was increasing the cardiac power, it was at the same time acting as a depresser of respiratory action, as shown by the breathing becoming slower, more feeble, and irregular, with occasional deep efforts to give relief.

In reviewing these facts, I have no hesitation in asserting that for an individual with suspended animation from drowning, whose respiration is not yet restored, the speedy employment of the continuous hot bath would be *wrong*, both theoretically and practically, tending all the more certainly and quickly to cause death—

1st. By increasing the congestion of the lungs;

2nd. By arresting lung action in more ways than one, to be hereafter detailed; and

3rd. By circulating poisonous blood in the system.

In these results I am remarkably confirmed by Dr. Waters, of Liverpool, in his 'Researches on Asphyxia'—a paper read before the Royal Medical and Chirurgical Society subsequently to the appearance in the 'Lancet' of the first portion of this article. And these conclusions, of which I hope shortly to show still further proof, are all in accordance with the views of the nature of drowning held by the ablest writers on the subject.

The hot bath, as the Royal Humane Society have used it of late years, for the treatment of drowning, is, then, an error, and a serious one.—*Lancet, Aug.* 10, 1861, *p.* 132.

152.—RULES FOR GUIDANCE IN CHLOROFORM ACCIDENTS.

By Dr. Kidd.

I. Young patients, up to fifteen, and females bear chloroform best; there seems a general "tolerance" of chloroform in large surgical operations, probably from the hemorrhage keeping the blood moving and preventing engorgement of the cavities of the heart; in females the reflex system is more active than in males.

II. Anæsthesia under chloroform is not at all of the nature of imperfect oxygenation of the blood. But this imperfect oxygenation may be superadded and run parallel with anæsthesia, leading to fainting fits after the administration; this is shown also by the plunging or violent efforts of the patient, from black blood in the muscles: even fresh concentrated vapour, from fresh additions to the inhaler, increases these muscular efforts, and always suggest the necessity of more air to the patient.

III. Where simple syncope is feared from fright, it is well to postpone the operation for a week, feeding the patient during this interval on meat and wine: the latter strengthens the muscles of the heart; it is well to bring such a patient under the chloroform, too, in a quiet ward rather than in the excitement of the operating-theatre.

IV. A passive, fixed, starting forward of the eyes, sudden contraction of the pupil, probably from spasm of the fifth nerve, and sudden change of the countenance, are the first signs of danger; the pulse may remain unchanged; chloroform should be kept warm (100° F.); this prevents coughing, which is also a dangerous symptom.

V. Simple syncope is much more dangerous than apnœa; it occurs, however, less frequently.

VI. The operations where nearly all the accidents have occurred have been those for removal of toe-nails, dead phalanges, tooth-drawing, strabismus, &c., operations on testis, reduction of dislocations, —all more or less connected with tendinous tissues.

VII. The right side of the heart, in apnœa cases, continues to beat long after the pulse stops. Probably while the iris of the patient contracts on approach of light we have a fair chance of waking up the entire heart: the right side of the heart will beat twelve or sixteen hours after the pulse at the wrist. Faradisation of the phrenic nerve or spine will set the heart in full action in animals through the renewed action of the respiratory muscles. Pulling out the tongue by forceps, so common hitherto in accidents, is a mistake; irritation of the nerves of deglutition stops the diaphragm.

VIII. Of 125 deaths, 54 occurred immediately before operation: 42 during small operations (syncope?); 25 from ether. The number of males in all returns seems to be exactly double that of females, though chloroform has been used in at least 30,000 cases of midwifery, and syncope is common in females. This leads to a suspicion that the deaths from chloroform are like railway accidents in the management of cases.

IX. Vomiting, as in cataract cases, is best prevented by an aloetic purgative beforehand an hour or two; the stomach also to be empty at the time of operating, and the chloroform pushed well to deep anæsthesia.

X. The "cardiac syncope" of Snow is partly a post-mortem change. These are all cases of apnœa, or where the mischief arises essentially from fixture of the diaphragm. The heart, in fact, pulsates longer than the lung will receive or return the blood—hence the congestion, increased also by the efforts made at resuscitation by the Surgeon himself.

XI. As to resuscitation, the means adopted at first should be as gentle as possible: a candle-wick gone out is to be blown in, not smothered by the *nimia diligentia*. Fanning the patient with cold fresh air is first in importance; the hands dabbed with cold water,

which has a powerful influence in exciting the brachial and heart nerves. Too much cold water is not advisable. The patient should be brought at once into the open air, if summer, or into a very warm room, if winter; turned on his right side; the soles of the feet and interior of the ear tickled with a pen; his left arm held up, smacked with a wet towel, and that axilla and side of chest dashed with cold water; then artificial respiration tried by "up and down" pressure rather than "rotations." Two or three needles next stuck, where the omo-hyoid lies at the outer edge of the sterno-mastoid, so as to hit off, if possible, the phrenic nerve; then the *moist* pole of a Faradisation apparatus tried over the part, or along the spine; the other pole inserted under the floating ribs.

XII. It is probable, in the milder cases, the previous measures will succeed, especially in the instances of apnœa; but if, after five minutes' trial, little progress is made, than tracheotomy is to be done, and a small tube (a No. 10 india-rubber catheter) passed, and air blown gently into the lungs (a large tube causes emphysema). Some ammonia and warm water should be thrown into the rectum. In syncope cases, transfusion into a vein, of warm water with a little soda in it may wake up the heart. In apnœa (asphyxia as regards the heart, the right cavities gorged), opening a vein is useful. These measures should be continued for at least four to six hours, the body kept warm by hot blankets in a semi-recumbent position, the battery, gases, and impure air of a crowd of students rigidly excluded; pure oxygen gas also to be avoided.—*Medical Times and Gazette, October* 5, 1861, *p.* 362.

153.—ON THE TREATMENT OF GOUT.

By M. Trousseau.

Lithontriptic lixivia and alkaline preparations were in high repute for gravel and gout as early as the preceding century; and even the waters of Carlsbad were recommended by Van Swieten. Nor can you be ignorant with what frenzy people have, in these times, pushed the use of the mineral waters of Vals, Vichy, and Carlsbad. My opinion is, that there does not exist in the world a more dangerous medication. I have, for my own part, certainly seen more than five hundred gouty persons who had been at Vichy, and had in consequence found themselves in wretched plight; and I do not know if, by way of compensation, my memory can furnish me with some isolated cases attended with real amelioration. Waters so strongly impregnated with alkali are inconsiderately prescribed by physicians, and are stupidly taken by patients. The danger thus incurred is often irremediable. M. Prunelle, who has for a long time practised medicine with much success at Vichy, was the first to point out the deplorable consequences of treating gout with alkalies *intus et extra*. But if, on the contrary, you have recourse to waters that are feebly mineralised

—those, for instance, of Pongues, Contrexéville, Plombières, Spa, or Wiesbaden, not only will you never see the occurrence of untoward symptoms, but you will remark in the great majority of cases a sensible amendment. When you have gravel as well as gout, Contrexéville and Pongues will give you therapeutic results of great value. The physician who really at this moment has the largest practice at Vichy thinks it is true that the waters of these celebrated springs are useful to the gouty, but in a very restricted measure; and hence he recommends their use for ten or twelve days only at a time. Alkaline saturation, in fact, appears to him a phenomenal expression of great gravity, and capable of occasioning a fatal result by unexpectedly provoking the appearance of atonic and visceral gout; and how many such examples has he not witnessed!

Peruvian bark was considered of such efficacy by Held, that he said, "Uno verbo, cortex peruvianus in podagra divinum est remedium." In our 'Treatise on Therapeutics,' M. Pidoux and myself have said, "It cannot be denied that, at the beginning of an attack of acute gout, sulphate of quinine, given in large doses, dissipates the pains and abridges the fits as certainly, at least, as those pernicious drugs known under a variety of names; but it is not necessary to grow old in the profession in order to understand, from sad examples, how carefully you should respect attacks of acute gout, and how great are the dangers which they incur who seek for sudden relief, so constantly purchased at a terrible price!

"But if in acute regular gout we proscribe Peruvian bark, as we proscribe other remedies, vaunted in the same circumstances, the case is altogether different when gout becomes vague and visceral, characterised by asthma, dyspepsia, disturbances of innervation—disorders of respiration, digestion, and circulation. Then it becomes useful from time to time to give Peruvian bark, especially when the patient, after abnormal gout, has fallen into a state of confirmed cachexia." The swelling, pain, and engorgement of the joints—the arthritic lesions and deformities—as well as, in a word, all the consequences of gout—yield readily enough to the action of the waters of Aix, in Savoy; but do not send thither a patient still labouring under gouty attacks, for you might in that way occasion some formidable visceral disorder. I do not, then, recommend a season at Aix, in Savoy, but to persons who *have had* gout, and now bear its indelible stigmata; while persons who still have the disease I direct to Wiesbaden, Pongues, Contrexéville, Plombières, or Spa.

Gouty persons, in order to protect themselves from humidity and atmospheric changes, have an evil habit of covering themselves immoderately with flannel. They would do better gradually to accustom their bodies to resist cold, and get habituated, as summer sets in, to washing in cold water; then, every morning on leaving bed, for a few minutes to wrapping in a wet sheet, the temperature of which should after a time be more and more reduced. The simple and ingenious

method of the Silesian peasant has furnished Boissière with some favourable results; and it has been well spoken of by M. Fleury. Hydrotherapæia, in fact, restores the cutaneous functions, prevents renal disorders, modifies consecutive symptoms, and communicates to the system a physiological impulse altogether new. Scudamore pretends that he could cite a multitude of examples to prove that the disposition to gout "has been effectually combated by having recourse in time to good advice, and by following a suitable mode of life and regular habits." We may say, in reference to this subject, that the prophylaxis of gout in a great measure consists in proportioning the quantity and quality of food and drink to the usual expenditure of the system, and to maintain the healthy condition of the renal, intestinal, pulmonary, and cutaneous excretions. [We think M. Trousseau might have added with advantage the Hippocratic rule, which to every one, without exception, enjoins *exercise* in proportion to the quantity of food taken.—Tr.] M. Boissière, moreover, has insisted much on corporeal exercise, and recommends to his patients walking, rubbing the *parquet* of their apartments, digging their gardens, sawing wood, &c., and dissuades the sons and descendants of the gouty from entering on any sedentary profession.

The gouty should avoid a diet consisting chiefly of meat, since the flesh of animals contains, in an equal volume, more albuminoid substances than vegetable aliment. Not only are the gouty liable to consume meat in too great quantity, but meats contain but few substances of an indigestible nature, and leave but a small amount of residue. Hence the peristaltic action of the intestine is not so much excited, and a state of habitual constipation may in this way be established. This fact did not escape the attention of Ambrose Paré; for he says, "the rich are tormented with gout more frequently than the poor, because they do not work, and they eat much and have diverse meats at all their repasts, and they drink in a similar manner and to excess, and are too much addicted to the game of draughts." Fresh vegetables, herbs, green legumes, and the red and watery fruits, as Boissière has well remarked, give rise to citrates, malates, and organic acids, which, in the system, become changed into alkaline carbonates, the effect of which is to render the blood and urine less acid. The ultimate transformation of animal matters, on the contrary, tends to a result altogether the reverse, and acidifies in a greater degree both the blood and the urine.

Should the physician deem it necessary to commence with immediately changing his patient's mode of life, he must, if he would avoid a check, or an aggravation of the disease, endeavour to accomplish gradually that change, especially should the gouty subject be old or enfeebled through his gouty diathesis. As muscular exercise, daily carried to the extent of causing slight fatigue, may, if gradual, be favourable; so harrassing the patient with violent efforts suddenly undertaken after a sedentary life and long-continued muscular inactivity,

may rapidly produce a fatal result. When it is once thoroughly understood that nothing should be attempted with precipitation, nothing exaggerated, then may your patient immediately commence with corporeal exercise.

Such is the picture of gout I proposed to lay before you. Like every chronic affection, it perverts the nature of the whole man; "homo integer deinceps nova inende refingendus." Physicians who listen to me, apply yourselves, then, to get a thorough knowledge of the disease.—*Medical Circular, June* 26, 1861, *p.* 451.

154.—*Formula for the Administration of Chloroform, Ether, Turpentine, Camphor, or Essential Oils.*—M. Vée recommends the following as a valuable means for the equable administration of these medicinal substances:—Chloroform (or any of the above) 4 parts (or less); oil of sweet almonds, 15 parts; powdered gum arabic, 10 parts; water, 100 parts; syrup, 25 parts. The chloroform is to be dissolved in the oil, and rapidly emulsified, to prevent evaporation. *Syrup of Chloroform* may be well prepared by the following formula:—10 parts by weight of chloroform are to be dissolved in 60 of oil of sweet almonds, 40 parts of gum added, and an emulsion formed with 350 parts of water. In this cold emulsion, placed in a close vessel, 540 parts of sugar are to be dissolved. It is a very stable syrup, rendering water white on admixture with it, and it contains exactly one-hundredth of its weight of chloroform.—*L'Union Medicale; and Med. Times and Gazette, June* 8, 1861, *p.* 607.

155.—ESSENCE OF BEEF.

By Dr. Druitt, London.

[Essence of beef is one of the old devices of cookery, and has been in use for a very long period.]

When meat, chopped up, and put into a closed vessel (such as an earthen jar, with a cover, sealed up with flour paste, and tied up in a cloth), is boiled or baked for an hour or more, it undergoes separation into three portions,—fat, solid fibre, and liquid essence. The last is easily strained off, and the fat separated by blotting-paper. It is, when nicely made, a clear, amber liquid, of a very meaty, aromatic smell and taste; but, as will readily be imagined, the quantity obtainable from a given weight of meat varies infinitely as the quality of the meat and the proportion of pure muscle, in contradistinction to sinew, varies. When made from old meat, it is darker; when from young meat, lighter; and, in this latter case, becomes gelatinous when cold. Almost one-fourth of its weight is yielded by ordinary meat.

Evaporated to dryness, it yields about one-sixteenth of solid residue; but this varies likewise.

The object of this preparation, in contradistinction to *common soup* or concentrated beef-tea, &c., is, to administer the purely stimulating elements of meat in a separate form. If all the soluble constituents of meat be extracted by boiling water, a gelatinous extract is obtained, far better adapted for most cases, whether in health or sickness; for the gelatine of meat has a very high nutritive value. But there are cases in which we wish to stimulate, and in which the stomach loathes a quantity of gluey matter, and for such cases this essence is adapted.

The object of my bringing it under the notice of the Obstetrical society was to advocate the use of it as a partial substitute for brandy, in the treatment of the consequences of hemorrhage, and other states of exhaustion incidental to the puerperal state. It will not supply the place of brandy during the early and urgent stages of exhaustion from loss of blood, though even then it is the best auxiliary, but it does very largely supply the place of an alcoholic stimulant in the treatment of the after stages, when the brain is suffering from anæmia, and when the patient complains of headaches, giddiness, and despondency, particularly in the early part of the day. It is equally valuable in states of mental depression arising from other causes, such as grief, mental shock, &c., in which the usual resort is to wine or brandy. It may be taken as a stimulant after breakfast, at the time of the day when nervous feelings are most distressing, and when alcoholic stimulants are most pernicious, and most likely, if used at all, to lay the foundation of injurious excess. Out of the vast number of drunkards, there are a few whose intemperance has followed the use of wine for the relief of mental depression caused by anæmia. Bodily weakness alone does not tempt to drinking, except when it is attended with mental despondency; but with that combination it does tempt most strongly. But the essence of beef, freed from the grosser gelatinous particles, is directly absorbed, and exhilarates the brain specifically, causing the same phenomena in slight degree as brandy does. It is in cases such as this, as a substitute for brandy, and as a means of preventing habits of drinking, and of curing them, that I more particularly recommend its use to my Professional brethren. One tablespoonful taken early in the forenoon, with or without a little bit of crust and a mouthful of wine, gives the brain a something that relieves it from nervousness and vapours. It should be given, not as food at meals, but in the guise of medicine, between them.

In dyspepsia, likewise, it is a capital remedy, and one infinitely preferable to pepsine, as I venture to think. There are very many cases of general debility and emaciation in which a gelatinous soup is preferable; but in the cases in which a nutritive stimulant without bulk is required, as in that hopeless disease called "old age," your readers may find that this liquid is a valuable addition to their stock of remedies. It may, if required, be flavoured with a little dill water or camphor, and administered out of a medicine bottle.

[Mr. Jardine Murray, of Brighton, considers that Dr. Druitt has been forestalled in the recognition of the value of this material, and in introducing it to the notice of the profession. Dr. Christison, in 1855, not only showed how it may be prepared, but how it may be preserved. The following is Dr. Christison's method :]

Cylindrical cases of tinned iron are filled each with six pounds and a-half of beef; and the lid is soldered on, but with a hole of about half-an-inch in diameter in the middle of it. Two trays of such cases are shoved into iron retorts, analogous in form to retorts for gas-making, but double-cased, so that steam may be introduced into the interstice around. They are thus subjected to a heat of 220° under steam pressure, for about three hours, by which the beef is partially cooked, and, being thus also made to contract strongly on itself, squeezes out a portion of its juice, emounting to a few ounces from each tin. The tins are then drawn, the juice is poured out, and the meat, with certain additions, is subjected to the preservative process. The juice, after being cooled and entirely freed of fat, is put into small four-ounce tin cases. Each of these has a small aperture at one end, which is secured with solder, after the juice is poured in. The tins are then subjected, on trays, to a temperature of 220° in a muriate of lime bath. On being removed, the solderer rapidly touches with his iron the solder on the top, which giving way allows steam to rush out forcibly and carry with it the air in the upper part of the interior. By the time he has thus swiftly passed over sixteen or twenty tins, the first is ready for being re-soldered by a similar dexterous application of his iron, which then in succession as quickly secures the whole open and steaming apertures. The process of heating in the bath, tapping, and re-soldering, is then repeated a second time, to make sure of the thorough expulsion of every particle of of air.—*Medical Times and Gazette, June* 1, *and May* 25, 1861, *pp.* 587, 560.

INDEX TO VOL. XLIV.

PAGE.

Abdominal collapse, Dr. Barnes on 322
Abortion, Dr. Priestley on cases in which the membranes and placenta are retained 237
Abscess, mammary, treatment of 366
——— pelvic, Mr. Skey on 149
Accidental separation of the placenta at the seventh month 239
Actea racemosa, Dr. Mac Donald on its use in acute rheumatism 12
Acute dysentery, its treatment by ipecacuanha 57
—— rheumatism, with unusual complication 13
—— rheumatism, on the use of actea racemosa in 12
Adams, Dr. J., on uterine hemorrhage 252
——— Mr. W., on anterior and posterior curvatures of the spine 105
Albuminuria, Dr. Beale on the urine in 66
Alcohol, Dr. Hooper on the action and uses of 386
Alcoholic stimulants in hospital practice, Dr. Gairdner on 383
Althaus, Dr., on electricity in amenorrhœa 357
Amenorrhœa, Dr. Althaus on electricity in 357
——————— Dr. Simpson on the local treatment of 358
Amputation, Mr. Teale on the advantages of the rectangular mode of .. 75
Anasarca, Dr. Witt's method of relieving 73
Anderson, Dr. T. McCall, on the diagnosis of psoriasis 217
——————— on the treatment of phthiriasis 200
Aneurism, Mr. Price on the treatment of 121
——— gluteal, Prof. Syme's case of ligature of 114
——— popliteal, treated by compression 114
Apoplexy, Dr. Chambers on blood-letting in 35
Arnott, Dr. J., on the treatment of stricture by incision 166
Arsenious acid as a substitute for quinine in fever 12
Arsenic smoking in asthma 47
Asphyxia, Dr. Watts on 371
Asthma, Dr. Julius on arsenic smoking in 47
Atrophy of children from dirt-eating 374
Auscultation, Dr. Chambers's "Drill" for 48

Barker, Dr. B. F., on shortening the first stage of labour 291
Barnes, Dr. R., on abdominal collapse 322
——————— on the induction of premature labour 231
——————— on the treatment of placenta prævia 241
Barton, Dr. J. K., his treatment of deformities from severe burns 190
——————— on excision of a portion of the os calcis 89
Beale, Dr. L. S., on the urine in albuminuria 66
Beatty, Dr., his case of dysmenorrheal membrane 362
Beef-tea as a medicinal stimulant 422
Bell, Dr. J., on the prevention of pitting in small-pox 188
—— Dr., on typhus fever 6
Benzine, its use in the treatment of scabies 199

PAGE.
Black eye, use of the root of the convallaria in 221
Bladder, Mr. Thompson on a new remedy for irritable conditions of 177
Blisters, Mr. Miles on the cure of gonorrhœa by 211
——— use of in the treatment of varicose veins and ulcers 116
Blood-letting in apoplexy, Dr. Chambers on 35
Blood-poisoning following scarlatina and parturition, Mr. Coulson's case of .. 315
Bouchut, M., on a pathognomonic sign of scarlatina 9
Breast, Mr. Nunn on inflammation of the 366
Brown-Sequard, Dr. C. E., his remarks on paralysis 25
——— ——— ——— on reflex paraplegia 18
Budd, Dr. W., his treatment of diphtheria 56
Bullet lodged in the heart for twenty years 124
Burns, Dr. Barton on the treatment of deformities resulting from 190
Bursal tumours, use of the thread seton in 89

Carey, Dr. F. E., on the treatment of delirium tremens by digitalis 29
Caries of the os calcis, and excision of a portion of the bone 89
Cartilages, loose, in the knee-joint 83
Catheterism of the eustachian tube, Dr. Voltolini on 221
Caustic potash, effects of its undue application to the uterus 337
Cerebral disease unattended by cerebral symptoms, Dr. West on 377
Cervix uteri, Dr. Greenhalgh on removal of the 336
Chalybeate, Dr. Skinner's new form of 399
Chambers, Dr. T. K., his "Drill" for auscultation 48
——— ——— on blood-letting in apoplexy 35
Cheesman, Dr. G., his case of extra-uterine gestation 301
Childs, Mr. G. B., his treatment of gonorrhœa 209
Chisholm, Prof., his operation for the radical cure of hernia 128
Chordee, Dr. Mouslon's treatment of 210
Chlorate of potash and sugar, efficacy of in phthisis 45
——— of potash injections in gonorrhœa 216
Chloroform accidents, Dr. Kidd's rules for guidance in 416
———, ether, turpentine, &c., formulæ for administering 421
——— its use in puerperal convulsions 255
Chronometry of life and disease, Mr. Walker on the 401
Cicatrices from burns, Dr. Barton on the treatment of 190
——— from suppuration of tubercular glands, treatment of 178
Clay, Dr. C., his observations on placenta prævia 248
——— ——— on ovariotomy and its results 338
Clover hay, its use as a remedy in hooping-cough 47
Coghlan, Dr. J., on dysmenorrhœa and sterility 364
Coliapse, abdominal, Dr. Barnes on 322
Collis, Mr. M. H., on a new mode of treating vesico-vaginal fistulæ 309
Colchicum, Dr. Hammond on the diuretic action of 409
Coma, on the causes of 27
Compression, popliteal aneurism treated by 114
Conservative surgery, a rare instance of 125
Convulsions and convulsive affections, on the causes of 28
——— ——— puerperal, Dr. Harper's cases of 253
——— ——— puerperal, Dr. Wilson's case of 256
——— ——— puerperal, use of chloroform in 255
Coulson, Mr. W., his treatment of flat ulcers of the rectum 149
——— ——— on hysteria masking secondary syphilis 350
——— ——— ——— on secondary abscess following scarlatina and parturition .. 315
Courty, Prof., M.A., his treatment of wens and other cysts 184
Crimea, Dr. Lyons on the deaths from fever in the 9
Crinoline accidents, Dr. Odling on 408
Cunningham, Dr. R. W., his treatment of acute dysentery 57
Curvature of the spine, anterior and posterior 105
Cysts, M. Courty on the treatment of, without excision 184

PAGE.

Davies, Mr. J. R., on the radical cure of varicocele 176
Decayed teeth, cases of furred tongue on one side arising from 52
Decidua, Prof. Virchow on a peculiar degeneration of 251
Deciduous membrane, Dr. Beatty on 362
Delirium and insanity, causes of 27
Delirium tremens, Dr. Carey on its treatment by large doses of digitalis .. 29
——————— —— Dr. Peacock on its treatment by large doses of digitalis .. 32
——————— —— Dr. Williams on its treatment by large doses of digitalis .. 31
Diabetic urnie, new mode of estimating the quantity of sugar in 67
Diarrhœa, Dr. Hynes on the efficacy of nitric acid and opium in 60
Digitalis, delirium tremens treated by large doses of 29
Diphtheria, Dr. Budd's treatment of 56
Diphtheritic ulcer of the uterus, Mr. Ellis on 329
Dirt-eating, Dr. Moore on the atrophy of children from 374
Disease, on the chronometry of life and 401
Dislocations of the shoulder-joint, Dr. Smith on 93
Drill for auscultation, Dr. Chambers's 48
Dropsy, Dr. Witt's method of relieving 73
——— following scarlatina, Dr. Taylor's treatment of 11
Drowning, Mr. Hunter on the treatment of asphyxia from 414
——————— inefficacy of the hot-bath in cases of 373
Druitt, Dr., on the preparation of essence of beef 421
Duncan, Dr. J. M., on the obliquity of the fœtal head in parturition 280
Dysentery, acute, Dr. Cunningham on its treatment by ipecacuanha 57
Dysmenorrhœa from mechanical obstruction, Dr. Coghlan on 364
Dysmenorrhœal membrane, Dr. Beatty's case of 362

Eastlake, Dr. H. E , on a case of impeded labour 298
Eczema, Prof. Hebra on the treatment of 201
Electricity and galvanism in amenorrhœa, Dr. Simpson on 361
——————— in amenorrhœa, Dr. Althaus on 357
Ellis, Mr. R., on diphtheritic ulcer of the uterus 329
——————— on inflamed ulcer of the uterus 332
Ergot of rye, is it dangerous or not to the life of the child? 264
Essence of beef, Dr. Druitt on the 421
Eustachian tube, Dr. Voltolini on catheterism of the 221
Excision of the knee-joint, Mr. Fergusson on a case of 88
——— of the os calcis, Dr. Barton's case of 89
Extension, its efficacy in treating contractions from burns 192
Extra-uterine gestation, Dr. Cheesman's case of 301
Eyelids, Mr. Teale's operation in cases of adhesions of 219

Farre, Dr. A., his case of induction of premature labour 236
Femoral artery, Dr. Rawson's case of traumatic aneurism of the 112
Fergusson, Mr. W., on excision of the knee-joint 88
——————— on Pirogoff's operation 92
——————— on the lateral operation in lithotomy 174
Ferri carbonas effervescens, Dr. Skinner's formula for 399
Fever, Dr. Latham on the difference between curing and treating 2
——— in the Crimea, Dr. Lyons on the deaths from 9
——— in the Zambesi, Dr. Livingstone on 1
——— typhus, Dr. Bell on 6
First stage of labour, Dr. Barker on shortening 291
Fistula, vesico-vaginal, Mr. Collis on the treatment of 309
——— vesico-vaginal and recto-vaginal, Mr. Spencer Wells on 306
Fœtal head, Dr. Duncan on the obliquity of the 280

PAGE.
Foot, Mr. Fergusson's case of removal of the 92
Forceps obstetric, Dr. Hamilton on their use in tedious labour 266
——— obstetric, Dr. Hewitt on a modified form of 269
——— obstetric, Dr. Sinclair on the timely use of the 272
Foster, Mr. M., his remedy for whooping-cough 47
Fracture of the radius, Dr. Gordon on the treatment of 97
Furred tongue, Mr. Hilton's curious cases of 52

Gairdner, Dr. W. T., on the use of alcoholic stimulants in hospitals 383
Galvano-caustic apparatus, Dr. Walker on 395
Gay, Mr. J., on incisions for stricture compared with dilatation 152
——————— on internal obstruction by bands 405
Gestation, extra-uterine, Dr. Cheesman's case of 301
Gibb, Dr. G. D., on saccharine fermentation of human milk 368
Gluteal aneurism, Prof. Syme's operation in a case of 114
Godfrey, Mr. J. J., his treatment of scabies by benzine 199
Gonorrhœa, Dr. Chambers on the treatment of 211
——————— Dr. Irvin on chlorate of potash injections in 216
——————— Dr. Mouslon on subnitrate of bismuth as an injection in .. 210
——————— Mr. Childs on injections of liq. hydrarg. pernitratis in 209
——————— Mr. Miles on its treatment by blisters 211
Gordon, Dr. A., on fractures of the lower extremity of the radius 97
Gout, M. Trousseau on the treatment of 418
——— M. Trousseau's medicinal treatment of 16
Gray, Dr. J., on laceration of the perineum during labour 259
Greenhalgh, Dr. R., on removal of the cervix uteri 336

Hamilton, Dr. G., on the use of the forceps in tedious labour 266
——————— on uterine hemorrhage 299
Hammond, Dr. W. A., on the diuretic action of colchicum 409
——————————— on uræmic intoxication 64
Harper, Dr. R., his three cases of puerperal convulsions 253
Heart, Dr. Balch's case of a bullet lodged in for 20 years 124
——— disease, its connexion with degeneration of the kidneys 63
Heath, Mr. C., on the treatment of stricture of the urethra 163
Hebra, Prof., on the treatment of eczema 201
Hemorrhage, uterine, Dr. Adams on the influence of contraction in stopping .. 252
——————— uterine, Dr. Hamilton on 299
Hemorrhoids, internal, Mr. Lane on the treatment of 138
Hernia, Prof. Chisholm's operation for the radical cure of 128
——— Mr. Wood's operation for the radical cure of 126
——— congenital inguinal, Dr. Wilson's complicated case of 130
——— scrotal, Dr. Agnew s operation for... 130
Heslop, Mr., his case of spontaneous evolution 295
Hewitt, Dr. G., on a modified form of obstetric forceps 269
——————— on supporting the perineum in labour 257
Hilton, Mr. J., his cases of furred tongue on one side 52
——————— on hysterical pains in the hip or knee-joint 351
——————— on the influence of rest in diseases of the joints 77
Holt, Mr. B., his treatment of stricture of the urethra 154
Hooper, Dr. D., on the action and uses of alcohol 386
Hooping-cough, Mr. Foster on a new remedy for 47
Hornidge, Dr. T. K., on the influence of ozene 399
Hospital practice, on the use of alcoholic stimulants in 383
Hot-bath, Dr. Waters on its effects in asphyxia 371
———, Mr. Hunter on its physiology, use, and abuse 413
Housemaid's knee, Mr. Skey's treatment of 88

PAGE.

Human milk, Dr. Gibb on vibriones in the 368
Hunter, Mr. C., on the hot-bath, its physiology, use, and abuse 413
Hunter, Mr. J., on irritation of the urethra in females 350
Hysteria masking secondary syphilis, Mr. Coulson on 350
Hysterical pains in the hip or knee-joint, Mr. Hilton on 351
Hynes, Dr. P. J., on nitric acid and opium in diarrhœa 60

Impeded labour, Mr. Murray's case of 296
Infants, on the weight of new-born 369
Inflammation of the breast, Mr. Nunn on 366
Inguinal hernia, Dr. Wilson's case of oblique congenital 130
Injections of chlorate of potash in gonorrhœa 216
——— of liq. hydrarg. pernitratis in gonorrhœa 209
——— of subnitrate of bismuth in acute gonorrhœa 210
Internal hemorrhoids, curative treatment of 138
Intestinal obstruction by bands, Mr. Gay on 405
Intestine, MM. Velpeau and Verneuil on perforation of the 135
Ipecacuanha, acute dysentery treated by large doses of 57
Irritable conditions of the bladder, new remedy for 177
Itch, efficacy of benzine as a remedy for 199

Joints, Mr. Hilton on the influence of rest in diseases of the 77
Julius, Dr. F. G., on arsenic smoking in asthma 47

Kidd, Dr., his rules for guidance in chloroform accidents 416
Kidney disease accompanied by albuminuria, Dr. Williams on 62
Knee-joint, Mr. Fergusson on excision of the 88
——— Mr. Hilton on hysterical pains in the 351
——— Mr. Square on loose cartilages in the 83

Labour, Dr. Barker on an effort to shorten the first stage of 291
——— Dr. Hamilton on the use of the forceps in 266
——— Dr. Hewitt on laceration of the perineum during 257
——— Dr. Murphy on supporting the perineum during 260
——— Dr. Skinner on rupture of the perineum during 262
——— Dr. West on the use of ergot of rye during 264
——— impeded from dorsal displacement of the arm 296
——— premature, Dr. Barnes on the induction of 231
——— premature, Dr. Farre's case of induction of 236
——— premature, Prof. Simpson on the induction of 227
Laceration of the perineum during labour, Dr. Gray on 259
——— of the perineum during labour, Dr. Hewitt on 257
——— of the perineum during labour, Dr. Skinner on 262
Lane, Mr. J., on some diseases of the rectum 138
Latham, Dr. P. M., on the difference between curing and treating fever .. 2
Lee, Mr. H., on syphilis 203
——— on the radical cure of varicocele 175
Leucorrhœa and vaginitis, Dr. Waller on 353
Ligature of internal hemorrhoids 139
Lin. aq. calcis on cotton wool for preventing pitting in small-pox 188
Liquor hydrargyri pernitratis injections in gonorrhœa 209
Lithotomy, Mr. Fergusson on the lateral operation in 174
Livingstone, Dr., on fever in the Zambesi 1
Loose cartilages in the knee-joint 83
Lymphatic glands, Mr. Price on tubercular disease of the 178

PAGE.

Mac Donald, Dr. F. R., on actea racemosa in acute rheumatism 12
Maisonneuve, M., on complete restoration of the tibia by the periosteum .. 103
Mammary abscess, treatment of 366
Mayne, Dr. R., on acute softening of the medulla spinalis 16
Medulla spinalis, on acute softening of the 16
Miles, Mr. H. C., on the cure of gonorrhœa by blisters 211
M'Nab, Dr. J., on sesquicarbonate of ammonia in scarlet fever 10
Moore, Dr. W., on the atrophy of children from dirt eating 374
Mouslon, Dr., his treatment of acute gonorrhœa 210
Murphy, Dr. E. W., on supporting the perineum in labour 260
Murray, Dr. J. J., on impeded labour from dorsal displacement of the arm .. 296

Nævus, Mr. Price on the surgical treatment of 117
Nichols, Dr. J., on a rare instance of conservative surgery 125
Nitric acid and opium, efficacy of in diarrhœa 60
——— its use in the treatment of prolapsus ani 141
Nose, Dr. Nichols' case of union of a completely separated portion of .. 125
Nunn, Mr. T. W., on inflammation or the breast 366

Obliquity of the fœtal head in the mechanism of parturition 280
Obstetric forceps, Dr. Hewitt on a modified form of 269
——— forceps, Dr. Sinclair on the timely use of 272
Odling, Dr., on crinoline accidents 408
Os calcis, Dr. Barton's case of excision of a portion of the 89
Otterson, Dr., on veratrum viride 392
Ovarian disease, danger attending pregnancy complicated with 324
——— disease, symptoms, diagnosis, and treatment of 344
Ovariotomy, Mr. Spencer Wells on 348
——— and its results, Dr. Clay on 338
Ozone, Dr. Hornidge on its influence on organic substances 299

Page Dr. F., on the use of chloroform in puerperal convulsons 255
Paralysis, Dr. Brown-Sequard on 25
Paraplegia, reflex, treatment of 23
Parturition, Dr. Duncan on the obliquity of the fœtal head in 280
Peacock Dr. T. B., on the treatment of delirium tremens by digitalis .. 32
Pediculi capitis, M. Devergie on the treatment of 200
——— corporis, treatment of 201
——— pubis, treatment of 201
Pelvic abscess, Mr. Skey's cases of 151
Perineum, Dr. Gray on its laceration during labour 259
——— Dr. Hewitt on its laceration during labour 257
——— Dr. Murphy on supporting during labour 260
——— Dr. Skinner on its laceration during labour 262
Perforation of the intestine, MM. Velpeau and Verneuil on 135
Pharyngoscope, Dr. Voltolini on the 221
Phthiriasis, Dr. Mc Call Anderson on the treatment of 200
Phthisis, Dr. Turle on chlorate of potash and sugar in 45
Phymosis, Mr. Price on the operation for 173
Pirogoff's operation, Mr. Fergusson's mode of performing 92
Pitting in small-pox, its prevention by lin. aq. calcis on cotton wool 183
Placenta, Mr. Stanger's case of accidental separation at the seventh month .. 239
——— prævia, Dr. Barnes on the treatment of 241
——— prævia, Dr. Clay on 248
Placental retention, Dr. Priestley on the treatment of cases of 237

	PAGE.
Pneumonia, Dr. Williams on the diagnosis of	40
Popliteal aneurism, its treatment by compression	114
Pregnancy complicated with ovarian disease	323
Premature labour, Prof. Simpson on the induction of	227
——— labour, Dr. Barnes on the induction of	231
——— labour, Dr. Farre's case of induction of	236
——— labour, Dr. Hall Davis on the induction of	233
——— labour, Dr. Priestley on the induction of	232
——— labour, Dr. Tyler Smith on the induction of	231
——— labour, Mr. Baker Brown on the induction of	232
Presbyopia, Mr. Wells on	220
Price, Mr. P. C., on the operation for phymosis	173
——— on the surgical treatment of vascular tumours	117
——— on tubercular disease of the lymphatic glands	178
Priestley, Dr. W. O., on abortion in which the membranes and placenta are retained	237
Procidentia uteri from perineal laceration, Dr. Roberts' case of	320
Prolapsus ani, Mr. Smith's treatment of certain forms of	141
Psoriasis, Dr. Mc.Call Anderson on the diagnosis of	217
Puerperal convulsions, Dr. Harper's cases of	253
——— convulsions, Dr. Page on the use of chloroform in	255
——— convulsions, Dr. Wilson's case of	256

Quinine, Mr. Turner on arsenious acid as a substitute for	12

Radius, Dr. Gordon on fracture of the lower extremity of the	97
Rawson, Dr. T. J., his case of traumatic aneurism of the femoral	112
Rectangular amputation, Mr. Teale on the advantages of	75
Recto-vaginal fistula, Mr. Wells on	306
Rectum, Mr. Coulson's tretment of flat ulcers of the	149
——— Mr. Lane on some diseases of the	138
——— prolapsus of, its treatment where the ligature is inadmissible	141
Reflex paraplegia, Editor of the British Medical Journal on	18
Reid, Prof., his case of acute rheumatism with unusual complication	13
Rest, Mr. Hilton on its influence on diseases of the joints	77
Restoration of the tibia by the periosteum after its removal	103
Rheumatism, acute, Prof. Reid's case of	13
——— acute, Dr. MacDonald on actea racemosa in	12
Roberts, Dr. D. L., his operation for procidentia uteri from perineal laceration	320
Roberts, Dr. W., on estimating the quantity of sugar in urine	67

Saccharine fermentation of the milk in the female breast	363
——— urine, new test for	72
Scabies, Mr. Godfrey on its treatment by benzine	199
Scarlatina, Dr. M'Nab on sesquichlorate of ammonia in	10
——— M. Bouchut on a pathognomonic sign of	9
Scarlatinal dropsy, use of benzoate of ammonia in	11
Scars from ulceration after suppuration of tubercular glands, treatment of	178
Scrotal hernia, Prof. Chisholm's operation for	128
——— hernia, Mr. Wood's operation for	126
Secondary abscess following scarlatina and parturition, Mr. Coulson on	315
——— syphilis, case of hysteria masking	350
Shoulder-joint, Dr. Smith's treatment of dislocation of the	93
Simpson, Dr. J. Y., on the induction of premature labour	227
——— on the local treatment of amenorrhœa	358
Sinclair, Dr. E. B., on the timely use of the obstetric forceps	272
Skey, Mr. F. C., his treatment of housemaid's knee	88

	PAGE.
Skey, Mr. F. C., on abdominal or pelvic abscess	149
Skinner, Dr. T., on rupture of the perineum during labour	262
——— on the ferri carbonas effervescens	399
Small-pox, Dr. Bell on the prevention of pitting in	188
Smith, Mr. H., on the treatment of certain forms of prolapsus ani	141
——— Dr. N. R., on dislocations of the shoulder-joint	93
Softening of the medulla spinalis, Dr. Mayne on	16
Speech, causes of loss of	26
Speculum, vaginal, Weiss' improved	305
Spinal marrow, Dr. Mayne on acute softening of the	16
Spine, Mr. Adams on anterior and posterior curvatures of the	105
Spontaneous evolution, Mr. Heslop's case of	295
Sputum, its uncertainty in the diagnosis of pneumonia	40
Square, Mr. W. J., on loose cartilages in the knee-joint	83
Stanger, Mr. G. E., his case of accidental separation of placenta	239
Sterility, and dysmenorrhœa, Dr. Coghlan on	364
Stricture of the urethra, Dr. Arnott on its treatment by incision	166
——— of the urethra, Mr. Gay on its treatment by dilatation	152
——— of the urethra, Mr. Heath on forcible dilatation in	163
——— of the urethra, Mr. Holt's cases of forcible rupture of	154
Subnitrate of bismuth injections in gonorrhœa	210
Sugar in diabetic urine, Dr. Roberts on estimating the quantity of	67
Symblepharon, Mr. Teale on the treatment of	219
Syme, Prof., his case of ligature of the gluteal artery for aneurism	114
Syphilis, Mr. H. Lee on	203
Syphilitic degeneration of the decidua	251
——— psoriasis, diagnosis from non-syphilitic	217

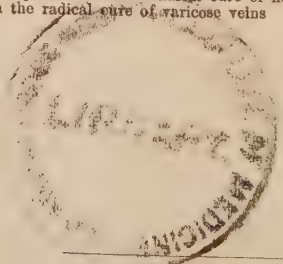
Taylor, Dr. J., on benzoate of ammonia in dropsy after scarlatina	11
Teale, Mr. T. P., on the advantages of rectangular over ordinary amputations	75
Teale, Mr. T. P., jun., on the treatment of symblepharon	219
Thompson, Mr. H., on a new remedy for irritability of the bladder	177
Thread seton, its use in the treatment of bursal tumours	89
Throat, ulcerated, treatment of	126
Tibia, M. Maisonneuve on its restoration by the periosteum	103
Tongue, furred, curious cases of, from various causes	52
Traumatic aneurism of the femoral artery	112
Trousseau, M., on the treatment of gout	418
——— his medicinal treatment of gout	16
Tubercular disease of the external lymphatic glands	178
Tumours, vascular, on the surgical treatment of	117
Turle, Dr. J., on chlorate of potash and sugar in phthisis	45
Turner, Mr. J., on arsenious acid as a substitute for quinine	12
Typhus fever, Dr. Bell on	6

Ulcerated throat, Mr. Westcott's treatment of	126
Ulceration, diphtheritic, of the cervix uteri	329
——— inflammatory, of the uterus	332
Ulcers of the rectum, treatment of	149
Uræmic intoxication, Dr. Hammond on	64
Ure, Mr., his case of varicose veins and ulcers treated by blisters	116
Urethra, Mr. Hunter on irritability of, in females	350
——— stricture of, Dr. Arnott on incision of	166
——— stricture of, Mr. Gay on dilatation in	153
——— stricture of, Mr. Heath on forcible dilatation in	163
——— stricture of, Mr. Holt on the immediate treatment of	154
Urine, diabetic, clinical method of estimating the sugar in	67
——— in albuminuria, Dr. Beale on the	66
——— new test for sugar in	72

PAGE.
Uterine hemorrhage, Dr. Hamilton on 299
——— hemorrhage, influence of contraction in arresting 252
Uterus, Dr. Greenhalgh on removal of the neck of the 336
——— Dr. Whitehead on the effects of undue application of caustic potash to 337
——— Mr. Ellis on diphtheritic ulcer of the 329
——— Mr. Ellis on inflamed ulcer of the 332

Vaginal speculum, Weiss' improved 305
Vaginitis, symptoms and treatment of 353
Varicocele, Mr. Davies on the radical cure of 176
——— Mr. Lee on the radical cure of 175
Varicose veins, Mr. Wood on the radical cure of 123
——— veins and ulcers treated by blisters, Mr. Ure's case of 116
Vascular tumours, Mr. Price on the surgical treatment of.. 117
Velpeau and Verneuil, MM., on perforation of the intestine 135
Veratrum viride, Dr. Otterson on 392
Vesico-vaginal and recto-vaginal fistulæ, Mr. Wells on 306
——— fistulæ, Mr. Collis's treatment of 309
Vibriones in human milk, Dr. Gibb on 363
Virchow, Prof., on a peculiar degeneration of the decidua 251
Voltolini, Dr., on catheterism of the eustachian tube 221

Walker, Dr. T. J., on the galvano-caustic apparatus 395
——— Mr. A. de N., on the chronometry of life and disease 401
Waller, Dr. C., on leucorrhœa and vaginitis 353
Waters, Dr. A. T. H., on asphyxia and the effects of the hot-bath 371
Weight of new-born infants 369
West, Dr. C., on cerebral disease unattended by cerebral symptoms 377
——— Dr. U., on the use of ergot of rye in labour 264
Wells, Mr. T. S , on ovariotomy 348
——— ——— on vesico-vaginal and recto-vaginal fistulæ 306
Wens, M. Courty's treatment of, without excision 184
Westcott, Mr. E., his treatment of ulcerated throat 126
Williams, Dr. C. J. B., on kidney disease accompanied by albuminuria .. 62
——— Dr. D. M., on the treatment of delirium tremens by digitalis .. 31
——— Dr. T., on the uncertainty of the sputum in the diagnosis of pneumonia 40
Wilson, Dr. F. W., his case of epileptic puerperal convulsions 256
——— Dr. W. J., his case of oblique congenital inguinal hernia 130
Witt, Dr C., on anasarca.. 73
Wood, Mr. J., his operation for the radical cure of hernia 126
——— ——— on the radical cure of varicose veins 123



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